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ASPECTS OF LINGUISTIC BEHAVIOUR

FESTSCHRIFT R B LE PAGE

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YPL records with great sadness the death, on 2 May 1980, of Dr M W S De Silva, editor of this special volume. Dr De Silva began planning this *Festschrift* as early as 1975 and was responsible for all aspects of editorial policy. At the time of his death, work on most of the contributions had reached first-proof stage. The editorial work has been completed by a small team consisting of John N Green, Christine Lockwood, Helen De Silva and Prasannajit De Silva.

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Robert B. Lefaye.

FESTSCHRIFT

R B LE PAGE

M W S De Silva
Editor

ASPECTS OF LINGUISTIC BEHAVIOUR

Today is 8 December 1980, and Professor Robert B Le Page is 60. The contributors present this volume to him with their best wishes in recognition of his academic integrity and intellectual stature.

My choice of Aspects of Linguistic Behaviour as an appropriate title for a Le Page *Festschrift* was motivated by two reasons. Firstly, Le Page studies language as a form of human behaviour and encourages the study of linguistics as a behavioural science. Secondly, 'Aspects of Linguistic Behaviour' is the title of a teaching course he introduced at York in 1964. This course, which is still being taught, characterizes a special attribute of York's linguistics department in that it aims to pull together the sociological and psychological aspects, transcending the traditional compartmentalization of these sub-components. The papers in this volume are particularly characterized by this last feature and do, therefore, constitute a fitting tribute to Le Page's approach to language. Some contributors have in fact applied the Le Pagean model to various linguistic situations and others have made interpretations of their findings in Le Pagean terms. The volume may thus be seen as coherent and intentional. All the papers published here were written especially for this volume. Those contributors who wrote particularly early sacrificed various opportunities of publishing their work elsewhere and waited for this day. I wish to record my deep sense of appreciation of every contributor's enthusiastic response to my invitation, and of the very complimentary things they have said of Le Page. I have to mention regretfully that David DeCamp died before he was able to complete his special paper for this volume.

Why does Le Page deserve a *Festschrift*? There are many reasons indeed, but the most significant one is the academic one, namely, that Le Page has developed an approach for exploring an important aspect of language in its social context which many others have at best merely recognized. Sudies of language as a formal system, as a system of communication and as a social phenomenon have frequently neglected the role of the individual as a determinant of language dynamics. It is to explore this neglected area that Le Page has devoted the past fifteen years or more. With his 'acts of identity' approach, Le Page has found it possible to complement significantly the observations of language change and language interaction as well as learning theory in general, without being harassed by such woolly notions as language, dialect, speech community and social motivation, and without necessarily postulating that language learning requires different sets of strategies from those employed in general learning.

I consider it appropriate to begin this volume with an account of Le Page's approach to language study. I think his 1976 Paper on 'Projection, focussing and diffusion...' gives the clearest exposition of his views. With his permission (received in devious ways) I, therefore, give below the full text of that paper as a fitting introduction:

Robert B Le Page
University of York

PROJECTION, FOCUSSING, DIFFUSION, or, steps towards a sociolinguistic theory of language, illustrated from the Sociolinguistic Survey of Multilingual Communities, Stages I: Cayo District, Belize (formerly British Honduras) and II: St Lucia.

In the multilingual and very fluid community of Cayo District, Belize we have recorded - among approximately 300 other children - one girl at the ages of both 12 and 18 years. On each occasion she told me a traditional Anansi story. Her mother was Mexican, her father Creole; she does not know what her own ethnic affiliation is except that it is mixed and that she is a Belizean. She feels that her native language is something called Creole, although it differs in some ways from the coastal Creole of Belize. On each recording, she starts by adjusting her behaviour in my direction - but her image of me changed over the years, since when she was 12 I was in some sense an Englishman who was probably a teacher, whereas when she was 18 I was her friend and she was a woman with a baby, living with a Government agricultural officer and so having a position to maintain in society. As she gets into the story she relaxes and recovers the vernacular of the playground of her childhood; as she gets more excited she recovers the story-telling register of perhaps her grandfather who told her the story. But at 18 she does not do this as easily and confidently as at 12; it is noticeable that there are more non-Creole forms. Finally, as the story comes to a close, she adjusts her behaviour once more to me.

I wish to be able to describe the behaviour of this girl and her peer group in such a way as to reveal something of the process of growing up in a fluid, multilingual society, of choosing an identity and a social role; to say something about the way in which the concept of what it means to be a Belizean in this newly-independent country is developing, is revealed by linguistic symptoms, and so on. I wish to do so in such a way that I may reveal something of general value about the way in which societies come into being; jell; and then dissolve; and the relationship of such processes to the historical and synchronic parameters of language.

This child's behaviour is idiosyncratic at each stage of her development, but at each stage reflects something of the stereotypes she has created in her mind about her own society. As she gets older, the stereotypes change, and probably harden. She is like other members of her peer group, but not identical to any of them. By being half Mexican and half Creole she straddles the two major cultural groups in the community; their Spanish is broken Spanish and their Creole broken Creole. Truly, therefore, both their language and their identity are emergent. I wish to study languages as social process. I think such studies may be important for developmental psycholinguistics, for sociology and for sociolinguistics; they may be important for linguistic theories of competence, too.

In recent years we have seen a number of notable attempts to reconcile linguistic theories based upon the concept of an idealized speaker-hearer in a homogeneous language community with the data for variable performance from sociolinguistic surveys. We have also seen notable attempts to reconcile within one theory synchronic and diachronic approaches to linguistic description. Much of this work has arisen from the study of the behaviour of Creole speakers in contact or diglossic situations, or of the behaviour of speakers of what is variously called Black English Vernacular or Non-standard Negro English.

I should like to mention just two of the descriptive procedures which have been proposed; to suggest briefly why neither of them seems sufficient - although each sheds useful light on the problems; and to propose an alternative theoretical approach which has been illuminating at least for me.

First, let us look at the kind of solution proposed by Labov in his studies of Black English. He makes the assumption that all members of the so-called English-speaking community must in some sense share a grammar; typically he derives divergent surface dialects from a common underlying grammatical structure to which rules are applied which not only embody the concept of linguistic variables to account for the lack of homogeneity in the individual, but which may also be marked as applying only in one dialect or another. He starts also with the concept of a standard language already firmly buttressed by educational norms, and frequently seems to be deriving dialectal forms from this, as in the case of contraction and deletion of the copula (Labov,1969). Deletion indeed is a common concept with him; in Black English not only is the copula deleted, but so also are the final dentals of verb past tense inflections. 'It is evident' says Labov ' that all the speakers of the English language treat the *-t,d* deletion rule in the same way, giving more recognition to the grammatical clusters in more formal styles and always preserving enough *-ed* clusters to demonstrate their knowledge of the language' (Language in the Inner City: 45). He argues carefully that Black English is *not* a separate system from the other varieties of English but can be accounted for by rules of the kind I have already referred to, even though it is distinct in several important grammatical categories involving tense and aspect. Labov's model is of a language community limited as to membership in an unspecified way, with a well-defined standard language, sub-groups of which - marked by their membership of socio-economic or geographical or ethnic groups - play their own linguistic variations on the underlying common theme. His descriptions are, moreover, always synchronic.

However accurate a sociolinguistic picture this may be for *his* communities neither the model nor the method of description will do for my purposes. My Creole speakers do not delete past tense inflection; it can be clearly shown that the Creole word is completely uninflected and that this has been the case for 200 years at least. Rather they *add* inflectional endings as they come under the influence of education, and they do so with many hypercorrections. At the same time they shift from an aspectual towards a tense-marking system. They do not delete the copula; the basic creole grammar does not

distinguish action predicates from stative predicates and has no copula. Although Belize Creole speakers may have fairly-well polarised norms of Creole speech, as one moves towards the Guatemalan frontier both Spanish and Maya have their influence, and there is no standard Creole. By Labov's definition hardly anybody in the country is an English speaker; yet the younger generation are all going to schools in which English is supposed to be the medium of instruction, and most would regard themselves as 'English speakers' in some sense.

It is not possible to start with Labov's assumptions, nor with the sub-groups already defined for us in socio-economic or ethnic terms. I will now look at Derek Bickerton's models and solutions (1973, 1975). He has put forward a new metatheory (1973), acknowledging his indebtedness to C-J Bailey, David DeCamp, Labov and myself. He takes linguistic variation as the *centre* - not the periphery - of language study; his metatheory breaks down, he claims, the Saussurean dichotomy between the synchronic and the diachronic. Language he sees as a dynamic process evolving through space and time, so that (I quote) '"leaky" grammars, variants that fit no system, conflicting native-speaker intuitions - all the problems that vexed previous formulations are now seen as the inevitable consequences of spatial or temporal segmentation of what is really a seamless whole' (1973: 643).

Bickerton describes usage within the Guyanese community as if it were that of an arbitrary chunk of the universal language community. He assumes that there are universal grammatical concepts such as 'personal pronoun' or 'aspect' which have differential exponents across the face and history of this community. Within the community there are isolects which can be defined through bundles of these differential exponents; for purposes of analysis selected variables can be arranged on an implicational scale. An isolect is 'any possible set of rules such that it will differ from adjacent sets of rules on a panlectal (implicational) grid only by a single rule-conflict or resolution of such conflict' (1973: 643). 'A PANLECTAL GRID may be defined as "the totality of possible sets of rules for an (arbitrarily limited) area in space and/or time, which in turn constitutes a selection from the totality of possible sets of rules for human language"'. 'A RULE CONFLICT may be defined as "a situation where, between an isolect A which contains a rule x and an isolect C which contains a rule y, there is a lect B wherein application of rule x alternates with application of rule y"'.

In this particular paper, Bickerton deliberately eschews giving a social correlation to his scales. "I merely wished to show' he says (p 666) 'that the mechanics of variation (as distinct, of course, from its social uses!) can be accounted for in purely linguistic terms'. He does however make use of William Stewart's terminology of acrolect, mesolect and basilect to refer to the most standard, the middle usage and the broadest vernacular usage in the creole continuum. He is drawing bundles of isoglosses in a grammatical space, relating them only to each other, to the grammatical forms of the model language and to universal grammatical concepts; locating groups of speakers in that space simply as the users of his isolects, without relation to geographical or social

features. The properties of his large corpus of utterances (taken to be representative of the Guyanese language in use) are held to arrange themselves into a linguistically-coherent traverse from basilect to acrolect; and this traverse is held also to reflect a language in a state of change from basilectal forms towards acrolectal forms, so that it is simultaneously synchronic and diachronic in nature, and therefore 'generative' (1975: 16, 18).

His development in this way of C-J Bailey's and David DeCamp's suggestions about grammars on implicational scales was avowedly an attempt to meet my own wish (expressed in B L Bailey, 1966: Foreword) for some kind of quantum linguistics to handle these creole/contact situations in a revealing way, to show language as social process. Quantum mechanics and concomitant theoretical developments, it will be remembered, brought to the study of the behaviour of very small particles the possibility of taking account of the effect of the observer on the observed; of making accurate but gross statistical statements about a large number of particles in spite of the apparently unpredictable behaviour of individual particles; and of allowing the views of matter as wave and of matter as particles to be reconciled through the realization that statistical statements about aggregates of particles were not statements about any individual particle (see eg Bunge,1967). Bickerton's approach however seems to have sacrificed a gain made, rather unwittingly in some ways, by Labov as early as 1966: the effect of the observer is no longer taken into account. Moreover, a continuum of lects is described, but not how a speaker is motivated to move across the continuum; it seems therefore that we have in Bickerton's theory no built-in way of reconciling wave-statements and particle-statements (that is, if you like, analogous to speech-act statements and isolect statements), nor any correlation between isolects and other social characteristics other than that of 'historical change'. Moreover, he polarizes his data into exemplifications of F1 (the 'basilect') and F2 (the 'acrolect') and speakers are then divided into those who consistently use F1, those who consistently use F2, and those who alternate between the two. This he does in spite of his criticism of the 'co-existing systems' approach to variability and his insistence that he is segmenting a continuum. His procedure raises acute theoretical problems, requiring *linguistic* constructs to be given a high degree of autonomy and speakers - once this has been done - to be regarded as more-or-less conforming exemplars of these autonomous 'language' varieties. As I have said elsewhere (Le Page,1972b: 123) 'the linguist ... jumps very easily from observations on an individual's usage to the creation of an abstraction for which he can write a grammar, and then back again to the individual seen now not as the source of his data but as an examplar, sometimes unreliable, of the grammar of the dialect the linguist has created'.

And so for my purposes, although Bickerton has taken a long stride to help me, he has not reached the position I wish to occupy. Although many historical linguists (eg Andersen,1973) would distinguish fairly sharply between socially-motivated and internally-motivated linguistic change, I find it impossible to do so, if for no other than the compelling reason that the abstraction we make and call 'the language' at any particular point is itself socially motivated. Let

give me an example: my Belizean informants twenty years ago appeared to an English-speaking observer to be moving backwards and forwards across a linguistic continuum between something like standard English and various kinds of coastal or inland creole usage; today the range of features is still very similar but there have been shifts in the *incidence* of more Spanish-like or more Creole-like or more standard-like forms, as the political and economic poles in the community have shifted. Any judgement about change in 'the language' will depend upon which speakers, in what context, observed by whom, provide the data for the abstraction; and statements about 'the language' should include statistical statements about functional load and frequency or probability of use.

Bickerton makes use of such statements, but only in order to polarize his texts as between *normal* - that is, fitting in with the overall pattern, so that eg normally acrolectal forms are not mixed with normally basilectal - and *deviant*, where such mixing *does* occur. His norm is one of invariant or partly-invariant speakers; any use therefore of Labov's or Sankoff's variable rules 'would simply obscure the considerable amount of invariant patterning ' in Guyana (1975: 18). Young, (1973) on the contrary, has clearly revealed what our own Belize data show - Belize Creole speakers all of whom are liable to use acrolectal forms (eg plural *-s*) *to some extent in any sample of speech*. Perhaps the difference is one of emphasis; perhaps on the other hand, as I suggest below, it could if theoretically well-founded reflect a real social difference between the two communities. The problem is, the soundness of the theoretical base. The soundness of Bickerton's theory seems to me to be very seriously impugned by the *ad hoc* arguments he constantly brings forward to maintain it in the face of contrary data (see eg 1975: 116 note).

To give another example, a recent study of Sikh immigrant children in Leeds (Rama Kant Agnigotri, 1979) shows them using a mixed code deriving from Punjabi, from Leeds English and from more standard English; the mix in any particular speech act depends partly on the speaker and partly on his audience - it is an instant pidgin, with very unpredictable features. My thesis is that in essence each and every speech act is like this: an act of identity towards an audience, an instant pidgin, the product of interaction, not simply of an individual's competence in the Chomskyan sense. Chomskyan theoretical constructs are unsuitable for the analysis of speech acts; Bickerton, by polarizing his data, is imposing upon it Chomskyan norms which belong to the description of *Language Three* not *One* or *Two* below, and which therefore to some extent obscure social processes. The modifications of generative theory which have been proposed to deal with variable performance data (eg by Labov, DeCamp, Bickerton) really amount to major modifications of the theory. (See eg Bickerton's 1975 re-definition of what 'knowing a rule' means - p 102.)

My own approach requires me to build into the units I abstract from observing the behaviour of the community, the social values attaching to the choice of variables. Moreover, it requires me to take note of the unpredictability of the individual in any particular

instance, so that observation can only be expressed statistically and in relation to larger chunks of behaviour than isolated sentences. It necessitates distinguishing at the outset between four meanings of the word 'language' which are often confused:

(i) the first reflects the rules which, we assume, must unconsciously motivate a speaker in his most relaxed and informal behaviour among his peers. This language, *Language One*, is not accessible to so-called 'intuitive' judgements, nor can it be observed; it is a hypothetical but necessary base. We do not even understand yet the nature of its 'rules'.

(ii) the second reflects the knowledge he has of the systems he attributes to various groups he identifies in his community from time to time, towards which he may be drawn or become averse. This I call *Language Two*. He may be more conscious of some of these groups, and of some of the rules, than of others; for example, in most communities some of the forms used by some groups are highly stigmatized by the socially-ambitious,while others pass unnoticed. There is not always, nor necessarily, a close correspondence between an individual's stereotypes and the actual behaviour of his model or aversive groups, even though both Labov and Trudgill have rationalized their own stereotypes about their own communities and found them well-based. If one asks a non-linguist informant about what is possible in his language he is most likely to answer with reference to the rules of a model group. Thus it is some variety of this Language Two which one explores through so-called intuition or experiments with test forms, and not Language One. In a completely homogeneous language community, of course, Language One and Language Two would be identical. However, there is never any such community; at the very least, young people want to differentiate themselves from old people.

Sociolinguists of my persuasion are concerned with Language Two. The socio-psychological theory envisages each individual in a community observing the behaviour of others, and extrapolating from observed variability towards various idealized models with invariable rules, the properties of which will reflect the general hypothesis and riders one and two in paragraph 8 below. In each speech-act the individual then positions himself in relation to his models; speech-acts therefore reflect mixed motives and capability according to riders three and four; variability within speech-acts will thus be seen as from within the actor - a variable mix of idealized invariable norms. Each speech-act is an announcement: 'to this extent I wish to be thought of as my own man, to this extent like A, to this extent like B, to this extent like C ...' and so on, where A, B, and C - and myself, and their properties, are the speaker's own constructs according to the general hypothesis and riders.

(iii) the third use of 'language', *Language Three*, reflects the activity of the theoretical linguist, making abstractions and writing rules, bringing to his observations of limited select data about past events one or more theories; using a metalanguage which, whether he likes it or not, cannot help embodying many of the properties he is testing for. He will tend to assume as innate, or as properties of all human language, logical concepts which probably should instead be thought of as reflecting unachievable goals *towards which* human languages aim. All that need be postulated as innate is the capacity to extrapolate from analogy towards logical targets in infinity.

(iv) *Language Four*, similar to Saussure's *Langue*, is again inaccessible. It is the linguistic properties inherent in the activities of a community - in their daily acts towards one another, their books, their grammars; having continuity of transmission daily, with concomitant incremental change of properties. One could liken it in some respects to the properties of a magnetic field; or to a co-operative bank whose members individually have a knowledge of their own transactions and balances but no complete knowledge of the overall balance sheet. The linguists are the auditors who never complete their audit. In fact, no final balance sheet ever could be drawn up; it is, once more, a purely hypothetical but socially - and psychologically - necessary construct. It exists because people need it to exist.

I have suggested elsewhere (eg in Le Page, Christie *et al*, 1974) a general motive for our individual linguistic behaviour, *viz*: that we create our 'rules' so as to resemble as closely as possible those of the group or groups with which from time to time we wish to identify. Our performance however is constrained by considerations which fall under one or another of four riders to this general hypothesis, that is

(i) the extent to which we are able to identify our model groups

(ii) the extent to which we have sufficient access to them and sufficent analytical ability to work out the rules of their behaviour

(iii) the strength of various (possibly conflicting) motivations towards one or another model and towards retaining our own sense of our unique identity

(iv) our ability to modify our behaviour (probably lessening as we get older).

In this connection I find it necessary to emphasize the phatic and self-expressive more than the communicative function of language. We engage in activities I call projection and focussing: we project on to the social screen the concepts we have formed, by

talking about them, so as to furnish our universe and try to get others to acknowledge the shape of the furniture; we in turn try to bring our concepts into focus with those of others, so that there is feedback from the social screen through language. A child who identifies easily with the stereotypes or concepts of a monolingual society will have a highly-focussed language system; the children I have been concerned with, in Belize or St Lucia or the immigrant Sikh children in Leeds, have much more diffuse systems. The focussing and diffusion that I speak of here are properties of Language Two or of Language Four, not the linguist's imposed focussing of Language Three. It is a great virtue of Bickerton's approach that he tries to let the data provide its own indices of focussing. Thus, if as according to his account Guyanese utterances exhibit grammatical properties which arrange them into three clearly distinguishable categories, both the language of Guyana and the linguistic behaviour of Guyanese is more highly focussed than that of Young's (or of our own) Belize and Belizeans. It is the task of the sociolinguist to try to explain such properties, and hence, the nature of language. (It is worthwhile recording here, anecdotally, the fact that when Young first came to the study of Belize utterances as a native speaker of Belize Creole but without training as a linguist he perceived them - heard them, understood them and wished to describe them - as much more highly focussed than when, three years later, he completed his thesis. To that extent therefore his Belize Creole was revealed as an idealized norm.) Through sociolinguistic training it should be possible to make Language Three a more accurate reflection of Languages Two and Four than otherwise.

In order to study the language behaviour of individuals within this framework it is necessary to establish at the outset units of description which are both linguistic and social. If I select a feature it will not be as part of the complete grammar of an abstract language - the possibility of writing a complete grammar is in any case a linguist's chimera - but as an attribute associated by some members of a community with the behaviour of others. If I may quote once again from Rama Kant's survey, he found that in general, the longer Sikh children had been in Leeds the more their language use resembled in certain features that of Leeds children. However, the analysis of the *reading* behaviour of the immigrant children reveals the operation of a constraint under my second rider. The indigenous Leeds children customarily in their relaxed speech have glottal stops where RP, for example, has voiceless alveolars or alveodentals in intervocalic or final position, and where these are written with ‹t›:

['bɛʔə] *better* ['bɛʔ] *bet* etc

When reading, these children replace the glottal stops with a feature of their own more 'refined' pronunciation, an affricated or assibilated alveodental eg [ˈbɛtˢə]. The immigrant children have very little access to or contact with the more 'refined' register of behaviour of the Leeds children, but their length of residence in this country correlates closely with the extent to which linguistically they adopt the *colloquial* features of the Leeds children in preference to those of Indian English or of RP; and

when reading, the longest-resident immigrant children as a group have a higher incidence of the *glottalized* forms than the indigenous children themselves. Thus, in their acts of identity, they use glottalized medial and final consonnants not only as allophones of a voiceless alveodental within their phonological system but also as a feature in the usage of a model group. We would not use this glottalized variety as one part of a sociolinguistic variable, because we would not for the moment be interested in what precise forms these children would be using if they were *not* modelling themselves on the Leeds children. We are only interested in that positive social act, the use of the glottal stop. Our aim, unlike Bickerton's, is not to write grammars, but to give quantified profiles of social acts so as to see a community through the eyes of the children growing up in it.

In selecting features to investigate therefore I must find those which carry social values in the community; and in describing the behaviour of individuals in terms of a quantification of such socially-marked features I am producing a psycholinguistic and socio-linguistic, and not a linguistic, description. However, the view of language which this work has led me to over the years seems to be most helpful in avoiding a number of the pitfalls which more abstract and autonomous views of language lead one into. It is this view which I have tried to express here, as also in 'The concept of competence in a creole/contact situation' (Le Page,1973). The survey results seem to illuminate the learning processes in children and the ways in which these are motivated; they appear to be socially conditioned in ways which are rarely taken into account by the linguist, and it is at this point - the hinge at which psycho-linguistics, sociolinguistics and general linguistic theory pivot in mutual interdependence, that we see our theory of language emerging.

The features which we select may well resemble Bickerton's in being *linguistically* as universal as possible - eg in phonology such features as nasalization or other assimilatory features; in morphology such as the marking of plurality or tense or aspect; in syntax SVO rather than SOV order, and so on; each however is marked from the outset with social significance. Labov, in eg Social Stratification of English in New York City, analyses the behaviour of his pre-determined socio-economic groups and *demonstrates* that in certain features one group's model for formal use will be the usage of another group. We, instead, initially assume this to be so, on the basis or prior observation, or discover it to be at least partly so on the basis of an intensive quantification of many properties of the first few tapes analysed. A pilot survey is essential for our purposes, for this and many other reasons; a period of one month at the end of which the experience gained with a small but we hope representative fraction of the final sample of children and families is dissected and the research plan then modified and settled accordingly. The very detailed study of many diagnostic features exemplified by the Tyneside Linguistic Survey (see Pellowe,1967) would be both impracticable and unnecessary for our purposes. To select our features initially it is sufficient that they turn out to be properties of in each case one or other reasonably large proportion of the community. In each case, the

usage of educated people as affected by both the spoken models of teachers and the written models of books provides one set of features; the usage of West Indian Creole English speakers another set. In Belize, the 'Spanish' community provide a further set; in St Lucia, the Creole-French- or Patois-speaking community. Even with such a crude triad of possible models the number of possible combinations is limitless, since each person's behaviour is quantified in the direction of each of these models, not expressed in terms of an *either/or* selection. By the end of the analysis we are left with linguistic features which in their quantified incidence reflect the usage of one model or another in the speaker's set of models - this then becomes their significance in the network of social relationships between members of a speech community.

The procedure which we followed in our Belize (British Honduras) survey has been described in two papers in Language in Society 1:1 and 3:1. The survey as a whole is conceived as a series of stages in which the same general theory is applied but the methods refined from stage to stage. The motivation of the survey as a whole is partly intensely practical: to help with teacher-training in the formerly Francophone and Anglophone colonial countries through a study of social and linguistic processes at work in those countries. Stage II has been carried out in St Lucia, again in the West Indies, but this time a formerly French- and Creole-French-speaking island. Stage III will we hope be carried out either in Cameroon or in Mauritius or Seychelles; meanwhile, similar studies are being made of immigrant children in Britain and perhaps in the future in France also. I should mention that the whole survey is a joint programme of the Language Department of the University of York and the Section de Psycholinguistique of the Université Louis Pasteur, Strasbourg, of which Madame Andrée Tabouret-Keller is the Director.

The work was originally envisaged and planned as a co-operative venture between linguists, psychologists, sociologists and social anthropologists, in which the insights of these disciplines would be mutually illuminating. It was also planned to take up to ten years or more. We hoped to keep the same research team together, constantly increasing their mutual understanding of each other's approach, so that a truly sociolinguistic discipline would in time emerge. In the event, the way in which SSRC research funding in Britain is arranged has militated against long-term programmes and as a matter of policy they are against keeping research teams together for long periods except within Research Units, which are becoming very difficult to establish. Professor Tabouret-Keller and I have to provide all the continuity ourselves, and train a research team afresh for each stage. Moreover, the results for each stage have to be achieved within two or three years or not at all. These limitations greatly inhibit serious research in Britain, but one has to accept them. Stage I was funded in the main by the British SSRC, the Ford Foundation and the French CNRS, and Stage II in the main by the French DGRST, except for the salaries of the co-directors which have of course been paid by their respective universities, York and Strasbourg.

The programme of elicitation, analysis and quantification of profiles for individual children is intimately related to a cluster-analysis programme, which allows us to cluster the children according to their sociolinguistic behaviour. Each child, we hypothesize, is himself as he grows up making something like a running cluster analysis of his society and assigning himself to successive groups according to

a) his wish to be identified with them

b) his assessment, in the light of the cues they give him, of his chances of being accepted by them

c) his wish to retain a separate identity for himself.

These are seen, in conjunction with genetic and environmental opportunities, as being the factors affecting learning, within the framework of my general hypothesis referred to above. The use of a cluster-analysis programme affects the kind of data we can collect, the form in which we collect it and the kind of analysis we make. So also do the practicalities of each situation. I wish now to discuss the design of our surveys within the constraints provided by the theory, the method of analysis and the practicalities. First however let me say that, although the collection of data for its own sake is highly unfashionable nowadays, nevertheless every item of information which we collect during the course of the survey contributes to our general insight into the significance of linguistic behaviour in children and the culture in which they are growing up. In running a teacher-training course, general insight into the culture of teachers and pupils is as valuable as the special insights provided by research results. For one thing, it helps to persuade the teachers that what we have to say is directly relevant to their situation. For another, it saves us from exaggerating the contribution which linguistics can make to solving the language problems of children. What can help is to provide teachers, and those who train them, with a tool for gaining insight into language and learning processes, and to show them some of the results of using such a tool.

Moreover, we need some checks on the general validity of results which will be based on a small sample of the population.

THE BACKGROUND AND SOCIO-ECONOMIC INFORMATION we collect therefore comes under the following heads:

(a) General background information

(i) Physical and historical geography of the country, and its physical relationships with its neighbours.

(ii) Livelihood: economic basis of the community, its economic structure and economic relationships; forestry, agriculture, fishing, industry, exploitation of other resources; economic geography and economic history.

(iii) Demography, both diachronic and synchronic; ethno-history; past and current migratory processes; political, religious and economic factors influencing demography.

(iv) The political system and ethno-political structure.

(v) The educational system and the resources available to it: whether and to what extent it is based on oral tradition or on literacy, on individual teachers or on schools; political control, bureaucratic structure, religious and lay government of schools, colleges and other institutions; distribution of these, their size, purposes, age-range catered for, syllabuses, examinations, annual intake, scholarships awarded, attendance records, fees paid, other costs to parents; finance available to governing bodies and political authorities; languages used or supposed to be used in instruction; recruitment and training of teachers, pay and inducements to teachers, social status of teachers, political involvement of teachers; overseas support (if any) and overseas connections of the educational system; ethnic, cultural, religious, linguistic and political biasses within the system; the structure of the teaching year, and of a child's educational career.

(vi) The religious beliefs, structures and activities of the community, and the extent of communal and individual involvement in these. Political aspects of religion and religious aspects of politics. Sanctions available on either side.

(vii) The general linguistic background: national and official languages; other languages commonly named as in use, with any officially-assigned or communally-assigned functions; linguistic prescriptions and proscriptions, official or communal. The functions of oral, and of written, literature in relation to each language or dialect of the community. Literacy, the media, and related statistics: circulation of newspapers in various languages, film audiences, broadcasting hours assigned to each within the territory, broadcasting in each language available from neighbouring territories; industrial uses of languages in advertising, instructions for use etc; book authorship, publishing, imports, sales and distribution; the role of literacy in the bureaucracy of the country (eg how many official forms, how complex, in what languages, who fills them in?). Rules and practices about evidence in police and other courts, etc.

Much of this information can be collected in advance. For example, the British Museum and other London libraries may have more complete sets of census reports, Blue Books, annual Education Department reports, Year Books etc than some of the former colonies themselves. Some of it however can only be collected, and all must be brought up to date, in the field. Demographic statistics are particularly tricky. In St Lucia we found that they are virtually non-existent, that the Government statistical office is short-staffed and out-of-date; our own statistics compiled from school registers are more

reliable than most that are available in the island. In this respect, starting a survey from the schools is a distinct advantage.

(b) Anecdotal accounts by members of the community relating particularly to the ethno-history of the community. As examples I would instance for Belize the following recorded accounts:

(i) What it used to be like and is like today in a logging camp or a chicle-tapping camp in the jungle.

(ii) What life used to be like in the girlhood of a woman who was born and has spent all her life in San Ignacio, before the road was built and when all the supplies had to come up from the coast by river.

(iii) An autobiographical account of a young woman who has grown up in various rural districts and in Belize.

(iv) A discussion with hotel waiters about the economic opportunities for them in Belize and in the United States, and with the hotel manager and his accountant about their employment policies.

(v) An account of his farm and his job by the local welfare officer in San Ignacio.

(vi) A number of recorded accounts by members of the families of children in the survey, about their lives and particular problems.

Similar recordings have been made with a number of St Lucian informants. These recorded interviews are only part of a much larger file of material containing notes made at or after interviews with a wide range of members of the community: farmers, shopkeepers, labourers, resident magistrates, head teachers, religious leaders, the nurse who had been in charge of a clinic for twenty-five years and so on. In spite of the claim once made by a well-known Canadian psycholinguist that he and his team could 'crack any culture in a weekend', we have for our part found that it takes months of assiduous talking to and observing all and sundry to get any real insight at all. I taught at the University of the West Indies for ten years, ran summer schools in various territories and have carried out a virtually continuous programme of Creole language research since 1951, but feel that I am still getting fresh insights into creole culture. In this respect, the 'alien' survey-worker is in a very different position from the Labov who surveyed the Lower East Side of New York or the Trudgill who surveyed Norwich. In St Lucia my team has consisted of Professor Andrée Tabouret-Keller as co-director and two French-speaking and French-trained research assistants (one of them from the Central African Republic), Mademoiselle Ghislaine Maury and Monsieur Marcel Diki-Kidiri. I had myself spent some time in the island years previously, and still had a few contacts there, but my team found it quite difficult for some time to get inside creole culture. In Belize the team consisted of Professor Tabouret-Keller, a Jamaican linguist (Pauline Christie), a Belgian psychologist (Baudouin Jurdant) and an expatriate English/Dominican anthropologist (Douglas Taylor). Again, it took time for

each of us to get inside the local culture. A good deal of social anthropology is involved here, and both psychologists and linguists tend to lack training in this field. As part of our survey we underwent three months of preliminary training in York, and in each case Professor Tabouret-Keller and I have spent one month with the team to start them off on their six months of field work. Unfortunately, as I have said, research grants in Britain and France are given normally for only two years, sometimes renewable for a third; without the benefit of prior knowledge of a community and its culture such a time-scale is inadequate, and even with prior knowledge one feels constantly lacking in adequate time. However, making use of all available opportunities to record or make notes of anecdotal material greatly helps in one's subsequent analysis of the sociolinguistic data.

(c) In-depth family studies We tried in Belize to give one member of the team - Jurdant - the opportunity to make a study in depth of five families by living with each of them for a time. In St Lucia personal contacts by members of the team (one of which at least is leading to marriage) achieved something of the same end. In most cases, fairly strong personal friendships have developed between individual members of the team and some of the children and/or families in the sample. There are of course dangers of biassed interpretation in such involvements, but the advantages probably outweigh these. It is however necessary to be on guard against an officious 'interpreting' local informant.

(d) The children's questionnaire

In addition to the interview - described below - each of the children was asked details of his language use with other members of his/her family - grandparents, parents, older siblings, younger siblings - and close schoolfriends; the questions were subcategorized as to topic - school matters, jokes, domestic matters etc. The child's claims in answer to these questions were subsequently analysed for comparison with the claims made by the older generation in answer to the family questionnaire (see Le Page, Christie *et al*, 1974: table 1), and Madame Tabouret-Keller is at present carrying out a further comparative analysis.

(e) The family questionnaire

(i) Belize. This was the responsibility of Andrée Tabouret-Keller and Baudouin Jurdant. It was designed to be filled in by a member of the team after an interview with the 'family' of each child - that is, the household in which he or she had mostly grown up. In the matriarchal and extended-family systems of the Caribbean the determination of this 'family' itself posed many problems. Frequently, a Creole 'family' consists of a nana, or elderly woman relative, looking after the house; a mother who has had children by a number of different men, one of whom may or may not be in residence at the time; and some of her children, together with other related children sent to board with the household for convenience when their own 'families' have come under stress for some reason. Many children change their 'household' from time to time. The mother is, in this kind of household, often

the chief provider; the fathers of the various children may or may not contribute. The 'extended matriarchal family' was not typical however of 'Spanish' households, nor of Maya households even though these households were also frequently very loosely-knit and transient.

The questionnaire was also intended to take account of the fact that some of the data would be more factual (as interpreted by the analyst) and some, matters of opinion by members of the household. Within the 'factual' information we had to distinguish between observed facts (eg what the house was built of) and reported facts (eg how much money the family spent at the shop every day). The matters of opinion (eg the ambitions of the older members of the household for the children, the political opinions of the household etc) were to be collected as much as possible from comparable people within each household, eg the mother. Here we ran into difficulties in Belize, since it depended very much on the type of household whether the mother or the father or the nana would be at home at the time we called. Since we wished to work during the hours of daylight (many houses having no electric lighting), needed to avoid very busy times (eg just before school in the morning; during the midday break) and had a great many interviews each to carry out, some latitude had to be given in the choice of informant within a household. Again, not all of us spoke Spanish. We had the questionnaires translated into Spanish, but sometimes a member of the household of a neighbour who spoke some English had to act as interpreter. We tried to achieve some uniformity in the answers to these questionnaires therefore but did not succeed.

The family questionnaire contained four kinds of question: those that could be answered by 'yes' or 'no'; those for which the answer could be quantified on a scale (eg 'always', 'often', 'sometimes', 'never'); those the answer to which would form part of an open set (such as 'who do you think is the most important man around here?'); and those designed to stimulate generally discursive answers.

(ii) St Lucia. The family questionnaire was a good deal revised for St Lucia in the light of experience in Belize, and of the experience of Baudouin Jurdant and Andrée Tabouret-Keller in the analysis of the answers. Moreover, virtually all the family interviewing in St Lucia was done by Marcel Diki-Kidiri, after the preliminary pilot survey done by all four members of the team had allowed the questionnaire and procedure to be still further refined. Uniformity in the elicitation of information about the families is however extremely difficult to achieve, and we have decided that it is best to acknowledge this fact and build in to our analytical procedures such adjustments as can allow for this. One of these is described in Le Page *et al*, 1974. In Belize, we found that the older members of the team could elicit more information

than the younger ones; that the women gained more confidence in some spheres and the men in others; that all of us gained expertise as the survey progressed; that we all tended to differ widely as to what constituted the best written information as a basis for subsequent analysis, from terse factual statements to lengthy impressionistic jottings. Andrée Tabouret-Keller and I appended to each questionnaire answer-sheet we completed an impressionistic vignette of the family. In St Lucia, Diki-Kidiri found his own African ethnic origin and command of better French than English mixed blessings; the St Lucians sometimes welcomed him for these qualities, sometimes found him difficult to place and hence were suspicious.

Underlying the preparation of the family questionnaire was the assumption that the main social parameters which might correlate with sociolinguistic clustering were: the child's age, sex, level of education, personal ambitions, intelligence; the geographical location of the household; the economic resources of the household in relation to family size; religious, political, educational, economic motivation of older household members; ethnic background; linguistic background; mobility of members of household; stereotypes current in the community; educational (in a broad sense) resources available to the child, the household and the community.

The manner of the Belize analysis has been described in Language in Society 3.1 (Le Page, Christie *et al*, 1974).

In both design and the analysis of the questionnaires we have been aware that we would at all stages be making subjective interpretations either of our own impressions or of other people's opinions, but in the St Lucian survey we have tried to make more allowance for this from the outset than we were able to in Belize.

(f) Local stereotypes

An experiment conducted in the Teacher Training College in Belize was designed to give us knowledge of stereotypical responses by teachers to children's voices, accents, dialects and languages. It was described in Language in Society 1.1 (Le Page, 1969). Essentially it consisted of playing short extracts from stories recorded from four informants of differing ethnic and linguistic backgrounds to the 119 teacher-trainees in the college, and asking them to answer the following questions about each child:

1) Is this a child you would expect to do well at school?
2) Is this a child you personally would enjoy teaching?
3) Guess the age and sex of the child.
4) Guess where the child grew up.
5) Guess the ethnic group the child belongs to.
6) Guess the child's first language, and state whether that is the same as the language the story is being told in.

No similar experiment was carried out in St Lucia but the field workers have been instructed to make notes as they go on any evidence of stereotypes and have interviewed with this in mind. The studies made under my supervision by Walter Edwards (1975) in Guyana

and by Rama Kant Agnihotri (1979) in Leeds have also paid a good deal of attention in their socio-economic questionnaires to social and linguistic stereotypes.

A rather different aspect of stereotypical responses is that which relates to the *polarization* of individual behaviour by various agencies. We have already noted that the effect of the observer must be taken into account in sociolinguistic theory. Labov has tended to deal with this problem by trying to remove the observer from the scene; by using interaction between members of various social groups as a way of getting at 'the real language' (see eg 'The linguistic consequences of being a "lame" ', in Labov, 1972). This procedure I feel relates to Language One, to the idea that there is some more consistent 'real' language than that evidenced in behaviour in the presence of an observer. Our own procedure is based on the supposition that all linguistic behaviour is stimulated by some context or other, and that that stimulated by a trained linguist is as valid a basis for observation as any other, provided that the underlying theory is adequate. Within this theory we are concerned with movement, eg from more formal to less formal, from identification with one group to that with another as the resultant of the effects of the various constraints of my four riders (set out above) changes. But just as there is highly polarized behaviour (eg responses of a class to a teacher's questions in school) in speech, so also there are highly polarized responses to such questions as 'What languages do people speak round here?', and by studying these we can discover something about the social stereotypes of the community. To give just one example, it is commonly believed in Belize that the teachers in rural schools are mostly Caribs. At one time there was some truth in this, but not today; nevertheless, the girl I referred to at the beginning of this paper confidently told me that most of the teachers in the Central Farm region were Caribs, in spite of the fact that as far as I could discover there were hardly any Carib teachers in that region at that time.

It has long been noted that teachers have powerful stereotypes about children, that these affect their expectations of the children's behaviour, and that the expectation affects that behaviour (see eg Cox, 1973). In other words, teachers' attitudes are among the cues available to the child about his chances of being accepted by various groups in his society, and so may affect his own motivation towards learning to acquire the supposed attributes of those groups. Evidence is beginning to emerge in St Lucia of urban/rural stereotypes, as in Belize and Jamaica and probably in Guyana. It seems that, even when St Lucia was almost 100% French- or Creole-French-speaking, an urban Creolized English began to develop, under the dominating cultural influence of nearby Barbados, in the port and capital of Castries. Among other features this urban variety is marked by the Bajan *does* and *did* as habitual markers of the verb, in contrast to the indigenous development of zero copula + *-ing* for both habitual and progressive:

'What do you/did you do for a living?'

Bajan	aɪ	dʌz sɛl ...	aɪ dɪd sɛl ...
St Lucian	aɪ	sɛlɪŋ ...	aɪ sɛlɪŋ ...

It is this kind of feature which is likely to influence the teachers in their response to their pupils in St Lucia (the Bajan forms have prestige), just as the use of consistently rural forms by one Creole girl in Belize led a number of the Creole (but urban) teacher-trainees to classify her as a child they did not want to teach, one unlikely to do well at school, and a *Carib* (ie of a social group low in their esteem).

THE SIZE OF THE SAMPLE

Both Labov (1966: 638) and Trudgill (1974: 27) have claimed that the 'structures of social and stylistic variation of language can be studied through samples considerably smaller than those required for the study of other forms of social behaviour'. I doubt this claim, believing it to be a *post-hoc* relationization of the limitation which time and opportunity impose on linguistic fieldwork and analysis. In Trudgill's case in particular, the smallness of his sample when broken down into categories of age, sex, social class etc leads to far too few informants remaining in each category for his graphs, diagrams and statistics to be consistently valid. Our own procedure has been to try to ensure that any social category in which we might be interested would have an average of five representatives in our sample on the basis of a random distribution. Thus, if we are interested simply in sex distinctions, a sample of 10 would suffice; if we now add three locational groups (rural, urban, main-road-not-urban) our sample needs to be 90; four socio-economic levels would bring it up to 360.

Against this however we too have had to take into account the practicalities of available time and resources. Were we able, as we hope we have been able in St Lucia, to restrict ourselves to morphological features, transcription would be possible for a larger sample than if phonological features have to be identified and quantified. Phonetic transcription for the latter purpose is extremely arduous.

One of the reasons in favour of both Cayo District, Belize and the chosen district of St Lucia was the size of population - in each case small enough for us to feel that the sample we would be able to interview and analyse would be reasonably representative of the population as a whole.

With these considerations in mind we decided to interview one in four of the children in the top three standards of all the primary schools and the lowest form in the one secondary school in Cayo District - totalling approximately 300 children - and one in seven in the chosen area of St Lucia, totalling approximately 150 children. In the case of St Lucia we deliberately interviewed one and a half times as large a sample as we intended eventually to work with, recognizing from the Belize experience that it was far better to begin with too much data and too many informants and to reduce to a sample of good recordings and reliable interviews than to find ourselves with gaps once the analysis had started.

The children were in each case chosen arbitrarily from the school registers. In the Belize survey the result was exactly equal numbers of boys and girls; in St Lucia approximately equal numbers - 70 boys and 80 girls.

A cluster-analysis programme needs a minimum population to operate on; for this further reason we have rejected samples as small as those argued for by Trudgill and Labov.

It is worth mentioning here a procedure designed as a check on the general validity of statements based on our Belize sample. A simple check-list of questions was put on to Paramount hand-sorted punched cards and distributed to every child in every class of the relevant age-groups throughout the country. Had the analysis of the answers shown results seriously at variance with those or our sample, we would have had to revise our procedures. Unfortunately, owing to poor communications, bad weather and no effective follow-up, many of the cards were lost and the check was valueless. I feel however that it may be worth trying again.

THE INTERVIEW

The recorded interviews with the Belize children were all carried out by Dr Pauline Christie in a manner already described briefly in Le Page, Christie *et al*, 1974. Normally the child left the school and sat talking to Dr Christie in the Mini-Moke vehicle some distance away. The tape recorder was running on the back seat, the microphone lay between them in the front seats. The topics for conversation were graded so as to help the transition from more formal to less formal responses - starting with school and school-work, going on to home, sports, cooking, holidays and so on. In addition to conversation, two stories were usually asked for: one traditional (an Anansi story), one from a story-book (eg The Three Little Pigs); and a passage was presented for reading, designed in advance to reveal as many linguistic features of interest as possible. It was intended that for analysis we would have each sociolinguistic feature quantified in each mode of behaviour: reading, formal conversation, informal conversation, telling story-book stories, telling Anansi stories. Thus with, for example, five features in five modes we would have for each child a profile of 25 quantities, a good basis for the cluster-analysis.

Unfortunately, many of the 'Spanish' and 'Maya' children were rather tongue-tied, or did now know any stories. For many of the Belize children therefore we have only achieved a profile of nine quantities - three features in three modes.

With the St Lucian children we followed the same general procedure but attempted to modify it so as to overcome these difficulties. For example, Mademoiselle Ghislaine Maury, who did all the St Lucian interviews, met each child twice, the first being a familiarization meeting. New snags however were encountered, some of them highly illuminating; for example, in spite of the great efforts of the teachers and the confident assertions of the senior Government officers connected with education, a very high proportion

of the children proved to be virtually illiterate. A prepared text (different from the Belize text, and designed (not wholly successfully) with St Lucian phonological and morphological features in mind) was nevertheless given to each child to read at the end of the first interview. Formality however tended to be resumed at the beginning of the second interview - perhaps less than at the beginning of the first. The most informal part of the interview - apart from the traditional story-telling (normally in Creole French) which concluded the second interview - has proved to be a discussion about local superstition.

Finding a place to interview the St Lucian children in peace and quiet was a problem; about half of them were taken back to the house where the team was living. Sometimes a room was made available in the school. Mademoiselle Maury recognises now that she would have obtained better results (a) if she had known the island better (b) if she had not talked so much herself. With some children the reading proved to be very slow and laborious. Thus, on average between one and a half and two hours was spent over each child interview, compared with one hour in Belize.

THE ANALYSIS OF THE DATA

The procedures have been described in some detail in Language in Society 1.1 (Le Page,1972a) and 3.1 (Le Page *et al*,1974), and I shall not dwell on them here.

The important stages are as follows:

(i) Transcription of a small sample of tapes in considerable detail.

(ii) Selection of features which appear to have definite social connotations, for quantification.

(iii) Transcription of all tapes and quantification of these features for all informants, generally in four or five modes of behaviour: eg reading, formal conversation, informal conversation, story-telling of various kinds.

(iv) *Cluster-analysis of sociolinguistic profiles of each informant* based on these quantities.

(v) Analysis of answers to the children's questionnaire and the family questionnaire.

(vi) 'Degree of association' tests between membership of sociolinguistic clusters and various factors tabulated from the family questionnaire.

(vii) Characterization of *clusters* as being apparently motivated by social factors F F1 F2 ... arranged in hierarchical order according to the results of the 'degree of association' tests in (vi).

(viii) Examination of individual cases that do not fit in with these general characteristics.

(ix) Independent examination of linguistic data and of social data for insights of general and theoretical importance - see eg Tabouret-Keller, 1975a and 1975b.

The examination of idiosyncratic cases is one of the most important parts of the survey - it is this which reveals most fully the complexity of the social motivation of children. In one Belize case, a young man found to have very consciously resisted creolization of his English whilst a pupil at St John's College in Belize, and to have accepted the degree of ostracism which this brought, had strong political motivation which was most revealing when we discussed it with him. In another case, it was simply that the family had moved from one district to another at a critical stage in a child's development. Madame Tabouret-Keller has analysed the semantic map of the terms used and of the claims made in relation to ethnic origin. In particular, a good deal of light has been thrown on 'split motivation' in behaviour; in Belize, in the tensions between 'how to behave if you want eventually to get a job outside the country' and 'how to behave as a good Belizean'; in Rama Kant's Leeds survey, in 'how to become a Leeds citizen without ceasing to be a Sikh'; in Walter Edwards's Guyanese survey, in his attempts to explain among other phenomena the retention of Creole speech habits and other Creole social behaviour in a rural area very close to the city of Georgetown.

REFERENCES

Agnihotri, Rama Kant
1979 Processes of Assimilation: A Sociolinguistic Study of Sikh Children in Leeds. Unpublished D Phil dissertation, University of York.

Andersen, H
1973 'Abductive and deductive change', Language 49: 765-93.

Bailey, B L
1966 Jamaican Creole Syntax, Cambridge: U.P.

Bickerton, D
1973 'The nature of a creole continuum', Language 49: 640-69.
1975 The Dynamics of a Creole System, Cambridge: U.P.

Bunge, M
1967 (ed.) Quantum Theory and Reality, New York: Springer.

Cox, S
1973 Teachers' Perception of and Attitudes to Regional Varieties of Spoken English. Unpublished B Phil dissertation, University of York.

Edwards, W
1975 Sociolinguistic Behaviour in Rural and Urban Circumstances in Guyana. Unpublished D Phil dissertation, University of York.

Labov, W
1966 Social Stratification of English in New York City, Washington DC: Center for Applied Linguistics.

1969 'Contraction, deletion and inherent variability of the English copula', Language 45: 715-62.

1971 '"The notion of system" in creole languages' in D Hymes (ed.) Pidginization and Creolization of Languages, Cambridge: U.P., 447-72.

1972 Language in the Inner City, Philadelphia: Pennsylvania U.P.

Le Page, R B
1968a 'Problems to be faced in the use of English as the medium of instruction in four West Indian territories', in J Fishman *et al*, Language Problems of Developing Nations, New York: Wiley, 431-41.

1968b 'Problems of description in multilingual communities', Transactions of the Philological Society 1968: 189-212.

1972a 'Preliminary report on the sociolinguistic survey of Cayo District, British Honduras', Language in Society 1.1: 155-72.

1972b Review in Linguistics 118.

1973 'The concept of competence in a creole/contact situation', York Papers in Linguistics 3: 31-50.

1975 'Polarizing factors: political, social, economic, operating on the individual's choice of identity through language use in British Honduras', in J-G Savard and R Vigneault (ed.) Les Etats Multilingues/ Multilingual Political Systems, Quebec: Laval U.P., 537-51.

Le Page, R B, P Christie, B Jurdant, A J Weekes and A Tabouret-Keller
1974 'Further report on the sociolinguistic survey of multilingual communities', Language in Society 3.1: 1-32.

Pellowe, J
1967 Studies Towards a Classification of Varieties of Spoken English. Unpublished M.Litt. thesis, University of Newcastle-upon-Tyne.

Richards, J C
1972 'Some social aspects of language learning', TESOL Quarterly 6.3: 243-54.

Stewart, W A
1965 'Urban Negro English: sociolinguistic factors affecting English teaching', in R Shuy (ed.) Social Dialects and Language Learning, Urbana-Champaign: NCTE.

Tabouret-Keller, A
1975a 'Un champ sémantique: les noms d'appartenance raciale au Honduras Britannique' La Linguistique 11.1: 123-33.

1975b 'Ethnic names and group identity in British Honduras'. Paper presented to the Annual Meeting of Society for Applied Anthropology (USA), Symposium on Sociolinguistics and Applied Anthropology, 21 March 1975 (to appear).

Tabouret-Keller, A and R B Le Page
1971 'L'enquête sociolinguistique à grande échelle', La Linguistique 6.2: 103-18.

Trudgill, P
1974 The Social Differentiation of English in Norwich, Cambridge: U.P.

R B LE PAGE: A BIOGRAPHER'S UNOFFICIAL ACCOUNT

Bob Le Page was born in 1920, the youngest of five children, and spent his childhood at Eltham in South London. The family was never well off, but Bob won one of the very few open scholarships to Christ's Hospital and he was a boarder with them at Horsham in Sussex until the age of sixteen, when, impelled by a sense that he needed a career, he became articled to a firm of accountants.

After the outbreak of war Bob joined the Fleet Air Arm becoming at first a navigator, later, after a variety of postings, an instructor. The Fleet Air Arm represented an escape from the penury of articles, and enabled Bob and Gina Rogerson to marry, though their three children were not born until several more years had passed. It seems, however, that even trainee accountants of comparative means must retain a sharpened sense of financial acumen, for while he was flying from a carrier escorting Atlantic convoys, Bob spotted an opportunity many men might have missed and he became (albeit briefly and unofficially) an importer of birdseed. This was plentiful in Virginia but in very short supply in the U K, and large quantities were ingeniously hidden for the voyage in the bombracks of the carrier's planes. The career implications of this act however were not followed through: instead of rising to the top of the accounting profession, Bob decided at the end of the war to act on his pilot's suggestion that he should take a degree, and the profits of his enterprise were directed into further education at Keble College, Oxford, where he read English Language and Literature between 1945 and 1948.

The two years which followed were spent as an assistant in Birmingham and tutor in Oxford while he worked at his doctoral thesis on the nature of Old English metre. Thus his earliest publications were on the interpretation of metre and on Old English metre, and it was essentially to teach English Language that he went as an assistant lecturer to the University of the West Indies in Jamaica in September 1950. While there, however, the focus of his intellectual concerns shifted. He developed the interest in creole which this volume celebrates, started a dialect survey of the West Indies and a series of creole studies, and undertook the dictionary of Jamaican English with Fred Cassidy.

A mere nine years later he was offered the new Chair of English at the University of Malaya at Kuala Lumpur, with responsibility for establishing a department. He spent over three years in Malaya as professor, and latterly also as Dean of the Faculty of Arts. While back in the U K on leave, however, he became involved in discussions which were taking place about the shape of the new university at York, and he was appointed head of the Department of Language here in 1964, thus initiating yet another phase of creative activity, both practical and intellectual.

Bob not only founded a department of unique scope which reflects his broadly based conception of the study of language as more than a narrowly linguistic discipline, he was also the presiding genius who set up one of York's colleges, Vanbrugh, and was its first Provost from 1967 to 1972. Creativity in the fullest sense seems indeed to be one of Bob's most central characteristics, as is his essential humaneness; both of these qualities are fully apparent to those who know him, and both are clearly shown by the intellectual and practical achievements of his varied career to the present.

PRINCIPAL PUBLICATIONS OF ROBERT LE PAGE

1951 'The dramatic delivery of Shakespeare's verse', English Studies 32.2: 63-68.

1952a The Structure of Alliterative Verse, Jamaica: University of the West Indies (mimeo).

1952b 'The English language', Caribbean Quarterly 2.2: 4-11.

1952c 'A survey of dialects in the British Caribbean', Caribbean Quarterly 2.3: 49-50.

1955 'The language problem in the British Caribbean', Caribbean Quarterly 4.1: 40-49.

1957 'A rhythmical framework for the five types', in A S C Ross (ed.) English and Germanic Studies, Birmingham: U.P.

1957-58 'General outlines of Creole English dialects in the British Caribbean', Orbis 6: 373-91, 7: 54-64.

1958 'The arts as a field of experience', in The Idea of a University, Jamaica: University of the West Indies, 73-91.

1959 'Alliterative patterns as a test of style in old English poetry', Journal of English and Germanic Philology, 58.3: 434-41.

1960a (ed.) Creole Language Studies I, London: Macmillan.

1960b 'Jamaican Creole: An historical introduction', in R B le Page 1960a: 3-124.

1961a 'General linguistics and English language studies', Universities Quarterly: 82-86.

1961b (ed.) Creole Language Studies II (= Proceedings of the Conference on Creole Language Studies, Mona, 1959), London: Macmillan.

1961c (with F G Cassidy) 'Lexicographic problems of The Dictionary of Jamaican English', in R B le Page 1961b: 17-36.

1962 'The language barrier'. Inaugural lecture, University of Malaya, Kuala Lumpur.

1964a The National Language Question, Oxford: U.P. for Institute of Race Relations. Reprinted 1966, 1971.

1964b 'Multilingualism in Malaya', in W H Whiteley (ed.) Proceedings of the 1962 Brazzaville Symposium on Multilingualism, London: CCTA-CSA Publications Bureau.

1967 (with F.G Cassidy) Dictionary of Jamaican English, Cambridge: U.P. Second edition 1980.

1968a 'Problems to be faced in the use of English as the medium of instruction in four West Indian territories', in J Fishman *et al*, Language Problems of Developing Nations, New York: Wiley, 431-41.

1968b 'Problems of description in multilingual communities', Transactions of the Philological Society 1968: 189-212.

1968c 'Intercomprehensibility between West Indian and other forms of English', in Remedial Education, Oxford: Pergamon, 129-35.

1968d (with P C C Evans) The Education of West Indian Immigrant Children, London: National Council for Commonwealth Immigrants.

1969a 'Dialect in West Indian literature', Journal of Commonwealth Literature 7: 1-7.

1969b 'Commentary on the theme, "How can we measure the effects which one language may have on the other in the speech of bilinguals?"', in L G Kelly (ed.) The Description and Measurement of Bilingualism, Toronto: U.P., 142-47.

1971 'The study of the English language', in A Ross (ed.) English: An Outline for the Intending Student, London: Routledge and Kegan Paul, 75-102.

1972 'Preliminary report on the sociolinguistic survey of Cayo District, British Honduras', Language in Society 1.1: 155-72.

1973a (with A Tabouret-Keller) 'L'enquête sociolinguistique à grande échelle. Un exemple: Sociolinguistic survey of multilingual communities Part I: British Honduras survey', La Linguistique 6.2: 103-18.

1973b 'The concept of competence in a creole / contact situation', York Papers in Linguistics 3: 31-50. Reprinted in D Feldman (ed.) Studia Gratularia in Honor of Robert A Hall Jr., Madrid: Playor, 1977, 173-89.

1974 (with P Christie, B Jurdant, A J Weekes and A Tabouret-Keller) 'Further report on the sociolinguistic survey of multilingual communities', Language in Society 3: 1-32.

1975a 'Polarizing factors: political, social, economic, operating on the individual's choice of identity through language use in British Honduras', in J G Savard and R Vigneault (ed.) Les Etats Multilingues /

Multilingual Political Systems, Québec: P.U. Laval, 537-51.

1975b 'Sociolinguistics and the problem of "competence"', Language Teaching Abstracts 8: 137-56. Revised as Le Page 1978b.

1977a (with A Tabouret-Keller, G Maury, M Diki-Kidiri, A J Weekes) 'Report to the DGRST Paris, on the Sociolinguistic Survey of Multilingual Communities stage II: St Lucia', Department of Language, University of York (mimeo).

1977b 'Processes of pidginization and creolization', in A Valdman (ed.) Pidgin and Creole Linguistics, Bloomington: Indiana U.P., 222-55.

1977c 'De-creolization and re-creolization: a preliminary report on the sociolinguistic survey of multilingual communities, stage II: St Lucia', York Papers in Linguistics 7: 107-28.

1977d 'Hugo Schuchardt's creole studies and the problem of linguistic continua'. Paper presented to the Schuchardt Memorial Colloquium, University of Graz.

1978a 'Projection, focussing, diffusion, or, steps towards a sociolinguistic theory of language, illustrated from the Sociolinguistic Survey of Multilingual Communities, Stages I: Cayo District, Belize (formerly British Honduras) and II: St Lucia', School of Education, University of the West Indies, St Augustine, Trinidad. Occasional Paper No 9 of the Society for Caribbean Linguistics. Reprinted in this volume.

1978b 'Sociolinguistics and the problem of "competence"', Language Teaching and Linguistics: Surveys, Cambridge: U.P., 39-59. (Revised and extended version of Le Page 1975b.)

1978c 'Some preliminary comments on comparative lexicography in the Caribbean'. Paper presented to the Conference of the Society for Caribbean Linguistics 1978, Cave Hill, Barbados.

1979a 'Theoretical aspects of sociolinguistic studies in pidgin and creole'. Paper presented at the Conference on Theoretical Orientations in Pidgin and Creole Studies, St Thomas, Virgin Islands.

1979b Review of Hymes, Foundations in Sociolinguistics: An Ethnographic Approach (London: Tavistock Publications, 1977) and Dittmar, A Critical Survey of Sociolinguistics: Theory and Application (trans P A M Seuren and K Whiteley, New York: St Martin's Press, 1976), Journal of Linguistics 15: 168-79.

1980 'The concept of "a language"', in K Sornig (ed.) Festschrift for N Denison, Graz: U.P., 174-92.

FESTSCHRIFT

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WHAT HAPPENS WHEN WE SWITCH?*

Throughout his work, Professor Le Page has emphasized the relationship between language and extralinguistic factors. While the importance of this relationship is now widely recognized — although all too frequently misunderstood, as I have tried to show elsewhere [1] — the precise ways in which it works have yet to be explored. In the present paper, I shall examine a single phenomenon which stands clearly at the interface between language and society: situational codeswitching in the creole-English continuum of Hawaii.

The data I shall use are drawn from a male working-class peer group in Puhi, a plantation village on the island of Kauai, Hawaii, whose language is a variety of Hawaiian (Creole) English. Members of this group were recorded in a variety of social situations, in fairly natural contexts (ie although some of the situations were directly occasioned by the investigator, they were situations such as might have occurred quite naturally in the lives of the subjects; moreover, except where specified, the investigator was not physically present). I shall concentrate on the outputs of two particular speakers, Vic, a 45-year-old lorry driver, and Sailor, a 48-year-old welder, since these were the most voluble speakers, and featured in most of the situations recorded. Some further facts about these two men may be relevant to what follows. Vic is easily the more sophisticated of the two, with a wide range of friends and acquaintances on several social levels; he is accepted as the natural leader of the group. Sailor, on the other hand, is rougher and less articulate, and tends to defer to Vic on most serious issues. However, both men play active roles in the *macho* working-class culture which revolves around boar-hunting, fishing, cockfighting and contact sports.

Both men were recorded while taking part in five different social situations, as follows:

(a) PG (Peer Group). Conversations while drinking beer with two or three other members of their group. Investigator not present.

(b) DY (Dyad). A conversation between Vic and Sailor, again while drinking, but with no other persons present.

(c) PW (Peer group + Wife). A conversation between Vic, Sailor and Vic's wife, while drinking coffee together, no others present.

(d) IP (Investigator + Peer group). Conversations similar to (a), except that the investigator was present (as participant observer and fellow beer-drinker) throughout.

(e) IPW (Investigator + Peer group + Wives). A conversation, over coffee, between Vic and Sailor, one other peer-group member, Vic's wife, the investigator and the investigator's wife.

If one were asked, *a priori*, to rank these situations in their order of formality, one's intuitive answer would probably correspond pretty closely to the order (a)-(e) given above, with the possibility that (since Vic and Sailor are close friends and have been so since childhood), (b) might rank as still less formal than (a). One would therefore expect the least formal speech in (a) or (b), and the most formal speech in (e). To some extent, these expectations are realized; it is the extent to which they are NOT realized with which this paper will be chiefly concerned.

In a creole continuum, the most obvious index of formality is the extent to which creole forms are replaced by their standard equivalents. This replacement is seldom complete; for that matter, the exclusive use of creole forms is almost equally rare. What happens in situational switching is that a kind of sliding scale operates. For those (extremely numerous) features which have both a creole and a standard variable, informality implies a high frequency of the creole variable and a low frequency of the standard variable; formality, naturally, implies the reverse relationship. This model will be adequate for the purposes of the present paper, although it should be emphasized that it represents an oversimplification of the facts; in many cases, there will also be intermediate forms, belonging neither to the standard nor the creole (Bickerton, 1973, 1975) which can in large part be accounted for by the difficulty of commuting surface features in grammars which differ widely from one another in structure. To introduce data of this kind would merely complicate the present exposition, which seeks to make certain points about the switching process *per se*.

One of the variables which characterize Hawaiian switching is *bin/wen* as a past marker.[2] For our two speakers, *bin/wen* behaves exactly as one might have predicted: that is to say, the percentage of *bin/wen* is highest in the least formal situation, and lowest in the most formal. In Table 1, the *bin/wen* percentage is in each case determined against the total number of past-reference verbs in the appropriate text(s).

TABLE 1

Ben/wen in five situations (Vic and Sailor)

	%DY	%PG	%PW	%IP	%IPW
Vic	50	31	28	22	8
Sailor	59	45	29	18	0

Note that, for each speaker, the fall is smooth and consistent across all five categories; even the figures of the two speakers are very similar, although Sailor starts with more *bin/wen* than Vic and finishes with less.

However, when we come to examine another creole variable, *stay* + V,[3] the picture is rather different. In Table 2, the *stay* + V percentage is in each case determined against the total number of progressive-aspect verbs in the appropriate text(s).

TABLE 2

Stay + V in five situations (Vic and Sailor)

	%DY	%PG	%PW	%IP	%IPW
Vic	100	58	33	41	0
Sailor	50	37	0	17	0

Here, for both speakers, the smooth decline in the percentages of *stay* + V is abruptly reversed in the IP situation. Instead of a figure predictably lower than that in the previous column, we have a higher one. The pattern is the more striking in that Sailor, in the PW situation, has zero *stay* + V. Surely, if Sailor completely abandons a form in two situations, and uses it in all others, these situations must have something in common which the others do not share.

In fact, Table 2 does not show an isolated case. If one takes a range of different features, standard as well as creole, one finds that their distribution will pattern either like that of *bin/wen*, in Table 1, or that of *stay* + V, in Table 2. Let us compare five features for Sailor, adding a sixth situation (I) a formal interview with the investigator. The five features are: (i) negation with preverbal *no*; (ii) *wan* as indefinite article; (iii) plural NP without surface marking of plurality; (iv) zero copula; (v) *om* as third-person object pronoun, singular or plural, as opposed to *him, her, it, them*. Figures in Table 3 are given as percentages of all negations, all indefinite articles, all plural NP, all copula-type constructions and all third-person singular and plural pronoun objects, respectively.

TABLE 3

Five features in six situations (Sailor)

	%DY	%PG	%PW	%IP	%I	%IPW
no-negation	65	94	72	55	5	34
wan-article	80	80	85	60	15	14
Ø-plural	22	21	5	27	6	3
Ø-copula	53	80	50	67	45	37
om-object	63	67	34	70	44	15

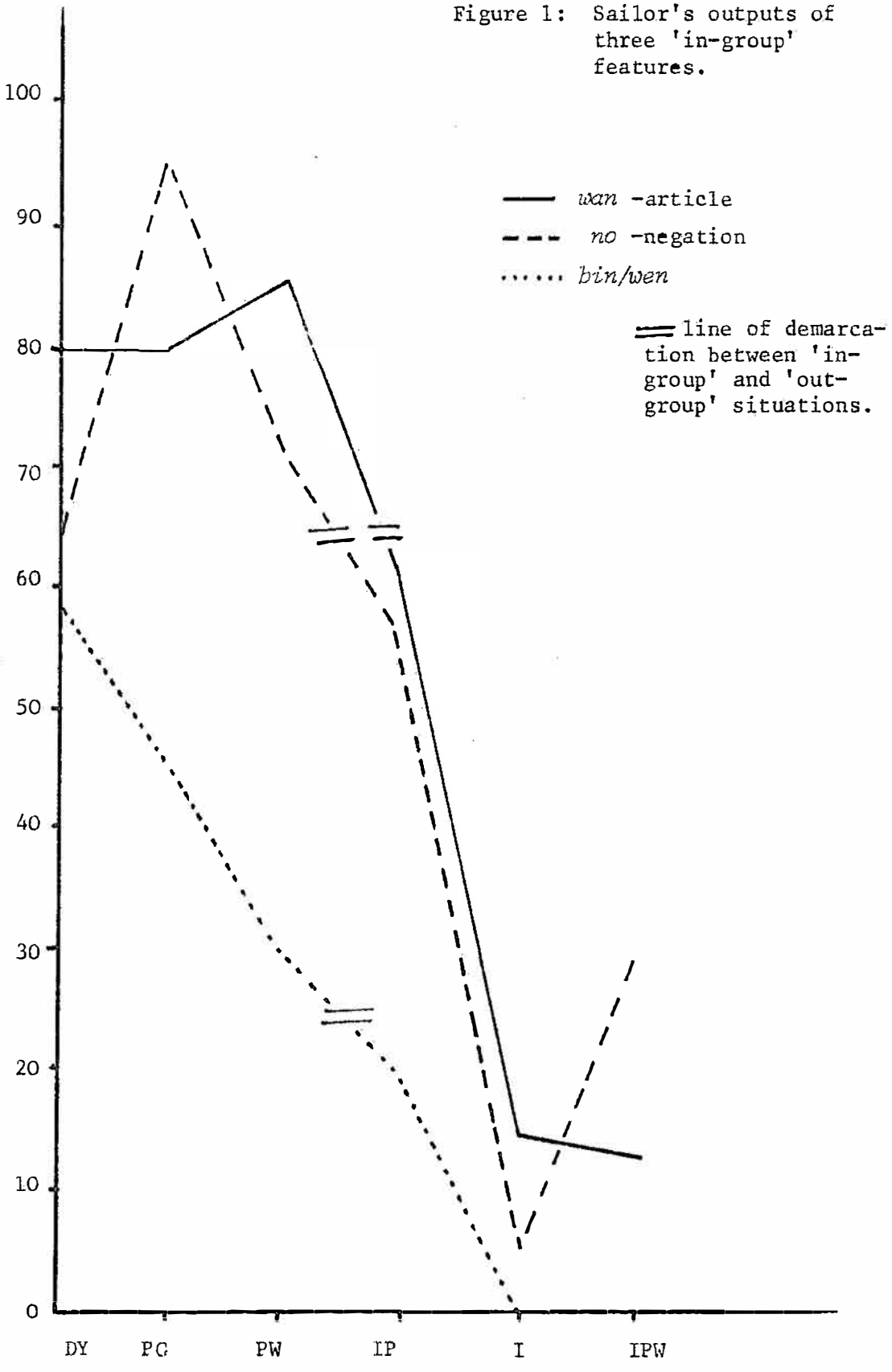
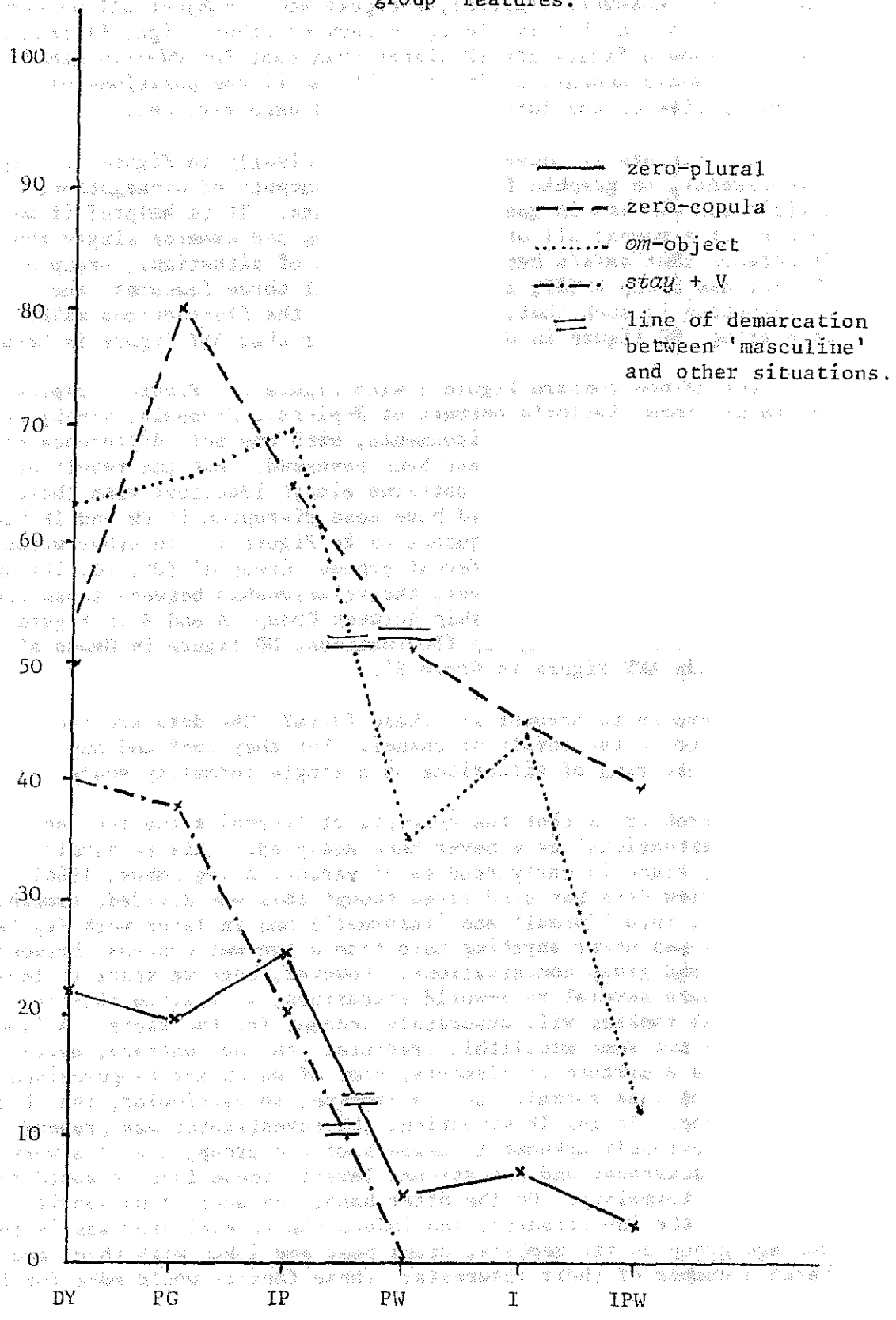


Figure 1: Sailor's outputs of three 'in-group' features.

Figure 2: Sailor's outputs of four 'masculine-group' features.



As Table 3 shows, *no*-negation and *wan*-article pattern like *bin/wen*, at least to the extent that they show a similar relationship between the PW and IP situations (ie PW higher than IP) — even though there are some slight fluctuations in the relationships of the other categories. However, Ø-plural, Ø-copula and *om*-object all pattern like *stay* + V, in that (while again showing other slight fluctuations) they all show a figure for IP higher than that for PW — in other words, it would appear, at first sight, as if the positions of these two categories on the formality scale had been reversed.

The picture is conveyed still more clearly in Figure 1. Figure 1 represents, in graphic form, Sailor's outputs of *no*-negation, *wan*-article and *bin/wen* in the six environments. It is helpful if we ignore for a moment all other fluctuations and examine simply the difference that exists between two groups of situations, Group A (DY, PG, PW) and Group B (IP, I, IPW). For all three features, the distribution is such that, no matter what the fluctuations WITHIN each group, NO figure in Group A is higher than ANY figure in Group B.

Let us now compare Figure 1 with Figure 2. Figure 2 represents, in graphic form, Sailor's outputs of Ø-plural, Ø-copula, *om*-object and *stay* + V in the same environments, with one sole difference — the positions of PW and IP have been reversed. But the result of that reversal is to preserve patterns almost identical with those of Figure 1 — patterns that would have been disrupted if PW and IP had been retained in the same sequence as in Figure 1. In other words, we now have two slightly different groups, Group A' (DY, PG, IP) and Group B' (PW, I, IPW); however, the relationship between these groups is identical to the relationship between Groups A and B in Figure 1, ie irrespective of intergroup fluctuations, NO figure in Group A' is lower than ANY figure in Group B'.

How are we to account for these facts? The data are too consistent to be the result of chance. Yet they confound any simplistic ordering of situations on a single formality scale.

The problem is that the concepts of 'formal situation' and 'informal situations' have never been analysed. This is hardly surprising, since in early studies of variation (eg Labov, 1966) only interview data was used (even though this was divided, somewhat arbitrarily, into 'formal' and 'informal') and in later work (eg Labov, 1969) there was never anything more than a two-way contrast between interviews and group conversations. However, once we start to look at and compare several real-world situations, we realize that no hierarchical ranking will accurately account for the facts. A 'social context' is not some monolithic creature: on the contrary, every situation is a mixture of elements, some of which may be perceived as more, some as less formal. Let us examine, in particular, the IP and PW situations. In the IP situation, the investigator was present - a person previously unknown to members of the group, and of a very different background and educational level; these factors would make for greater formality. On the other hand, the peer group heavily outnumbered the investigator, and indeed the investigator was in the same age group as its members, drank beer and joked with them, and shared a number of their interests; these factors would make for less

formality. In the PW situation, only persons of a similar education and background, who had known each other for many years, were present; these factors would make for less formality. On the other hand, the sexes were mixed, and the representative of the opposite sex was the wife of the group leader; these factors would make for greater formality. In both situations, then, one could argue that the signals were crossed, and might well predict that the linguistic outputs would reflect this, as they do.

However, this cannot be quite the whole story. For two of the features discussed, the PW situation has a figure not merely lower than that of IP but lower than that of I, the interview situation, as well! For these two features, the grouping is very clear. A' has figures of 27, 22 and 21 against B', 6,5 and 3 for the first feature, ∅-plural, and figures of 70, 67 and 63 against 44, 34 and 15 for the second feature, *om*-object. Group A' contains just those situations which (a) are all-male (b) include more than one peer-group member (c) feature communal beer-drinking, as distinct from Group B' which contains the two situations in which women were present as well as the most obviously artificial of the six situations, the interview. On the other hand, Group A contains just those situations which include only in-group members, whereas Group B contains just those situations which include out-group members as well.

It would appear, then, that situations can be perceived along two dimensions; one dimension being that of the all-male *macho* drinking culture versus all others, the other being that of in-group versus out-group. The data here is clearly insufficient to serve as the basis for any broad generalizations about the relationships between such perceptions and linguistic behavior, yet it suggests the hypothesis that varying perceptions are indeed reflected, and are reflected precisely along the lines of the dimensions we have discussed.

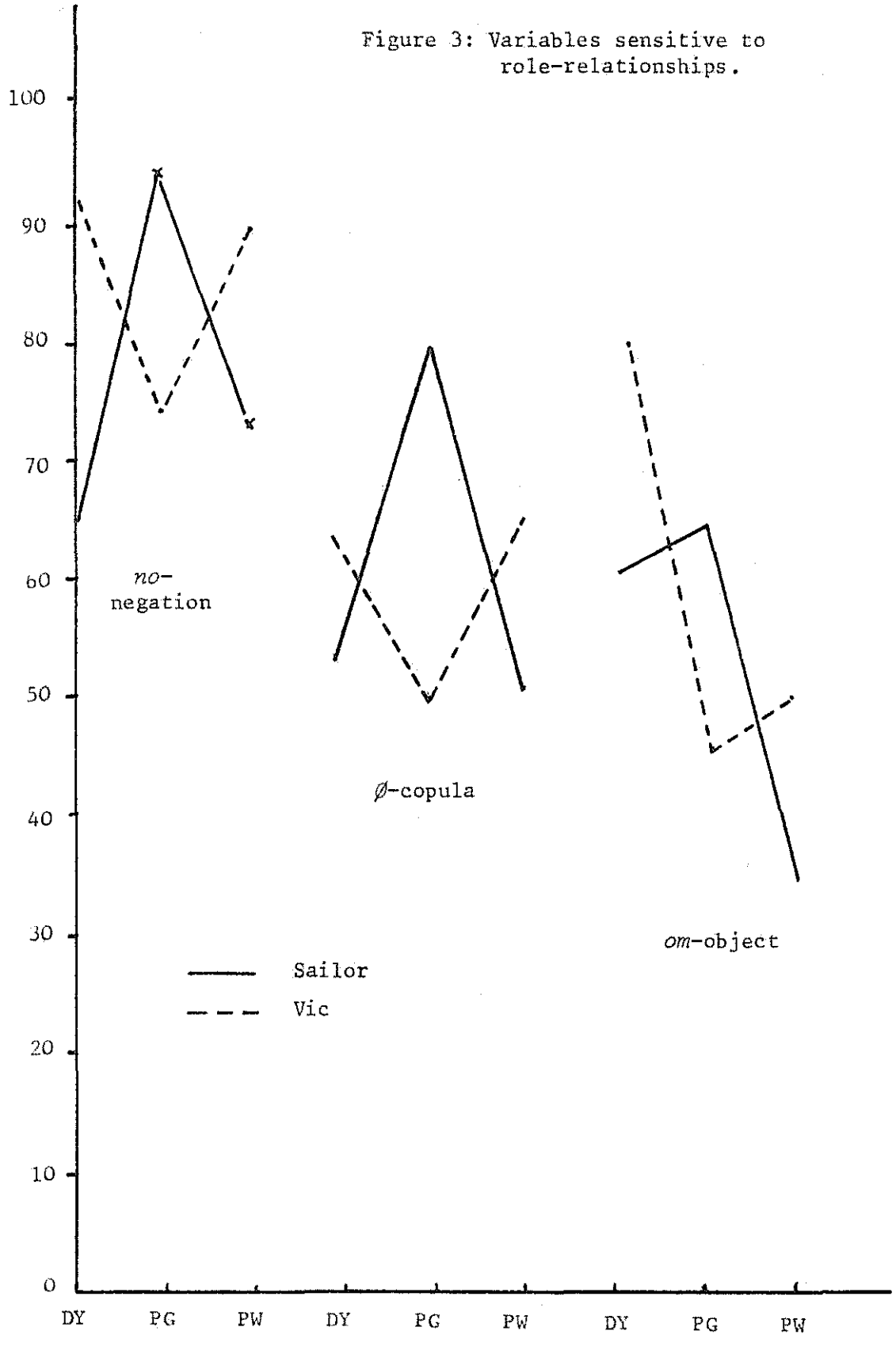
We have hitherto ignored the fluctuations affecting DY and PG. However, there is evidence that these too are not due to chance, but to the working of regular principles that govern human interaction. If we refer once more to Table 3, we observe that, for three grammatical categories, instead of the predicted fall in total from DY to PG, there is actually a rise — slight in the case of *om*-object, but amounting to nearly thirty percentage points in the cases of *no*-negation and ∅-copula. However, the significance of these figures does not emerge until we compare Sailor's output in these three categories with Vic's, as in Table 4 below.

TABLE 4

Three variables in three situations (Vic and Sailor)

		%DY	%PG	%PW
no-negation	Sailor	65	94	72
	Vic	93	73	89
∅-copula	Sailor	53	80	50
	Vic	62	50	64
om-object	Sailor	63	67	34
	Vic	80	46	50

Figure 3: Variables sensitive to role-relationships.



In each grammatical category, Vic's behavior is the exact mirror-image of Sailor's. In each case, Sailor's highest figure is in the PG situation, with, in all cases, markedly lower percentages in the other two situations. Conversely, Vic's lowest figure is always in the PG situation, with the other two categories, in all cases, showing markedly higher figures. The relationship can best be shown graphically, as in Figure 3. How can we account for this curious patterning?

Again, the only problem lies with the fact that concepts of 'social situation' have not been sufficiently refined. In all the literature I know of, situations are treated as if they were the same for all their participants. A moment's thought will show the unreality of any such supposition. A situation between two persons of highly asymmetrical status may be formal or informal. If it is formal (a criminal and his lawyer in a courtroom appearance, for example), both persons will switch upwards, although perhaps to different extents. If it is informal (say, the same criminal and lawyer, celebrating the former's acquittal), then the former may still switch upwards, but the latter will switch downwards. The second situation will be differently perceived by its participants: by the criminal, perhaps, as 'hobnobbing with the nobs', by the lawyer as 'a spot of slumming'. Unless role-differences (which may be much subtler than in the rather gross example just given) are taken into account, the very concept of 'situation' becomes sterile and unreal.

If we examine the data of Table 4 in this light, it immediately makes sense. It is true that the dyadic relationship of old friends is perhaps the closest and most intimate — more so than the peer group with its inevitable cross-currents and divided loyalties. However, as indicated by our brief comments on their personalities at the beginning of this paper, the status of these two friends is far from equal. Sailor looks up to Vic and respects his leadership; thus, no matter how close they are, there must be at least a touch of 'minding his p's and q's' in Sailor's attitude. Vic, on the other hand, can relax completely in the company of his faithful old henchman. But in the group, their relative roles are reversed. Here, Sailor can relax and be one of the boys. Vic, on the other hand, has to assume his mantle of group leader, which includes acting as mediator between the group and the outside world. The topics the group discuss, in addition to adventures and misadventures in the realms of sport, sex and hunting, include such things as recent trade-union negotiations, the position of sugar and pineapples in the world market, and the right way to raise children in the permissive climate of today. The more abstract the topic, as pointed out in Bickerton, 1973: 662-63, the 'higher' the language, but this burden does not fall equally on all; it is Vic, in the role of leader, who has to formulate the attitudes of the group towards the various issues, and Vic, in consequence, who has to use a higher proportion of standard forms than his fellow-members.

However, the relative status of the two men switches again when we come to the third situation, PW. It will be noted that, in all three categories, Vic's output of nonstandard forms is higher in PW

than Sailor's. But once again, their roles in the situation are different. For both of them, it is true, PW involves interaction with a member of the opposite sex — but to Sailor, that member is the wife of his group leader, whereas to Vic, it is his own wife! Naturally, in these circumstances, Vic is more relaxed than Sailor, and, equally naturally, his greater relaxation is reflected in the production of more tokens of nonstandard variables.

The foregoing suggests that there are two quite different ways in which situations can differ from one another. A situation may have ambiguities built into it, as it were, so that the same person may see it as more or less formal, depending on the point of view from which he is looking at it: this was true for situations PW and IP in the present study. Then, a situation which may or may not be intrinsically ambiguous may be perceived differently by different participants as a consequence of the different roles they play in that situation: this was true of situations DY, PG and PW in the present study. Both types of situational perception may, according to the data shown here, be reflected in language use.

However, it may be argued that if Vic's and Sailor's perceptions of the DY, PG and PW situations differ as we have suggested, then their linguistic outputs should ALL pattern like those in Figure 3. If — with respect to certain features — they do not, then the crossed-V pattern of Figure 3 could surely be attributable to chance.

If our hypothesis about the relationship between Vic's and Sailor's roles is correct, however, it is not necessary that the crossed-V pattern should be reproduced in every case. All that is necessary to prove that hypothesis is to show that Vic either always has a higher percentage of the nonstandard variable in DY and PW but a lower one in PG, OR HE WOULD HAVE IF SOME SPECIFIC GRAMMATICAL FACTOR DID NOT INTERVENE. We will now show that apparent counter-examples to the hypothesis can in fact be accounted for by differences between individual grammars.

The crossed-V pattern of Figure 3 can only exist if the overall frequencies of a feature are approximately the same for both speakers. However, there is no need to suppose that such frequencies are necessarily similar, even for speakers who are members of the same peer-group. Indeed, the grammars of such speakers can differ on points much grosser than mere frequency rates. In the present case, Vic has at least one grammatical feature which Sailor does not have at all. There is a Hawaiian regionalism, largely confined to Kauai, but not shared by all members of the Kauai population, which consists of using the morpheme *had* as a past marker. Vic has it with a fair degree of frequency; Sailor does not have it at all. If grammars of close associates can differ with regard to possession or non-possession of particular features, it should hardly surprise us that they can differ with regard to overall frequency rates. If we refer to Table 2, we find that Vic's overall frequency rate for *stay* + V is considerably higher than Sailor's in every category except IPW (where neither speaker has *stay* + V) — never less than 20 percentage points in any category. Obviously, if the overall disparity for a given feature

is as high as this, a crossed-V pattern will not result, even if the predicted fluctuations between the various situations continue to occur. However, if one makes a uniform adjustment for the disparity, either a crossed-V pattern or some other configuration that would fulfil the condition (Vic higher in DY and PW, lower in PG) should emerge.

In fact, such an adjustment will produce the appropriate pattern for any of the other variables we have examined. For *stay* + V and *wan*-article, Vic produces significantly higher proportions of the non-standard variable than Sailor. Their actual outputs, graphed, are shown in Figures 4a and 5a. However, if we adjust Sailor's pattern by increasing his output 30 percentage points for *stay* + V and 10 percentage points for *wan*-article, we get the result shown in Figures 4b and 5b: Vic's output, in both cases, remains higher for DY and PW, but is now lower for PG, as predicted. For *bin* + V, the reverse is the case: Sailor produces significantly higher proportions of the non-standard variable than Vic. Their actual outputs, graphed, are shown in Figure 6a. But if we again adjust the output of the speaker with the lower output — this time Vic — by adding 10 percentage points in all three categories, we obtain the result shown in Table 6b: once again the condition with regard to the relationship between the variables is fulfilled.

Admittedly, the number of percentage points by which outputs were raised is in each case an arbitrary figure, and it might be objected that, if another figure had been chosen, another result might have been obtained. For instance, if, in Figure 6a, we had raised Vic's *bin/wen* output by 5 rather than 10 points, his total would have been higher than Sailor's in PW and lower in PG, but also lower in DY, contrary to prediction. But this fact is no more relevant than the fact that, if we had advanced Vic's figures by 20 points instead of 10, he would have been ahead of Sailor for all three situations. The point is that THERE IS AN ADJUSTMENT VALUE (in this case 10 points) WHICH DOES SATISFY THE CONDITION. There are, as a moment's thought will show, any number of possible data configurations in which the condition could NOT be satisfied, irrespective of the adjustment value chosen. Thus the fact that an appropriate configuration shows up for each of the variables examined is far from being a matter of chance distributions. This, in turn, suggests that switching behavior is governed, not so much by the nature of SITUATIONS (as generally assumed in the literature hitherto) but by the ROLES of speakers in those situations.

If this finding can be replicated in other studies, it should have some interesting implications. Hitherto, one might have concluded that languages which grammaticalized or lexicalized role- or status- differences in the discourse of socially asymmetric pairs (such as Japanese or Javanese) did so merely as a consequence of possessing highly stratified and hierarchized societies — that, in other words, the possession of 'politeness morphemes' and the like represented a highly marked convention, far removed from any kind of linguistic 'naturalness'. However, confirmation of the present findings would suggest that the linguistic acknowledgement of social

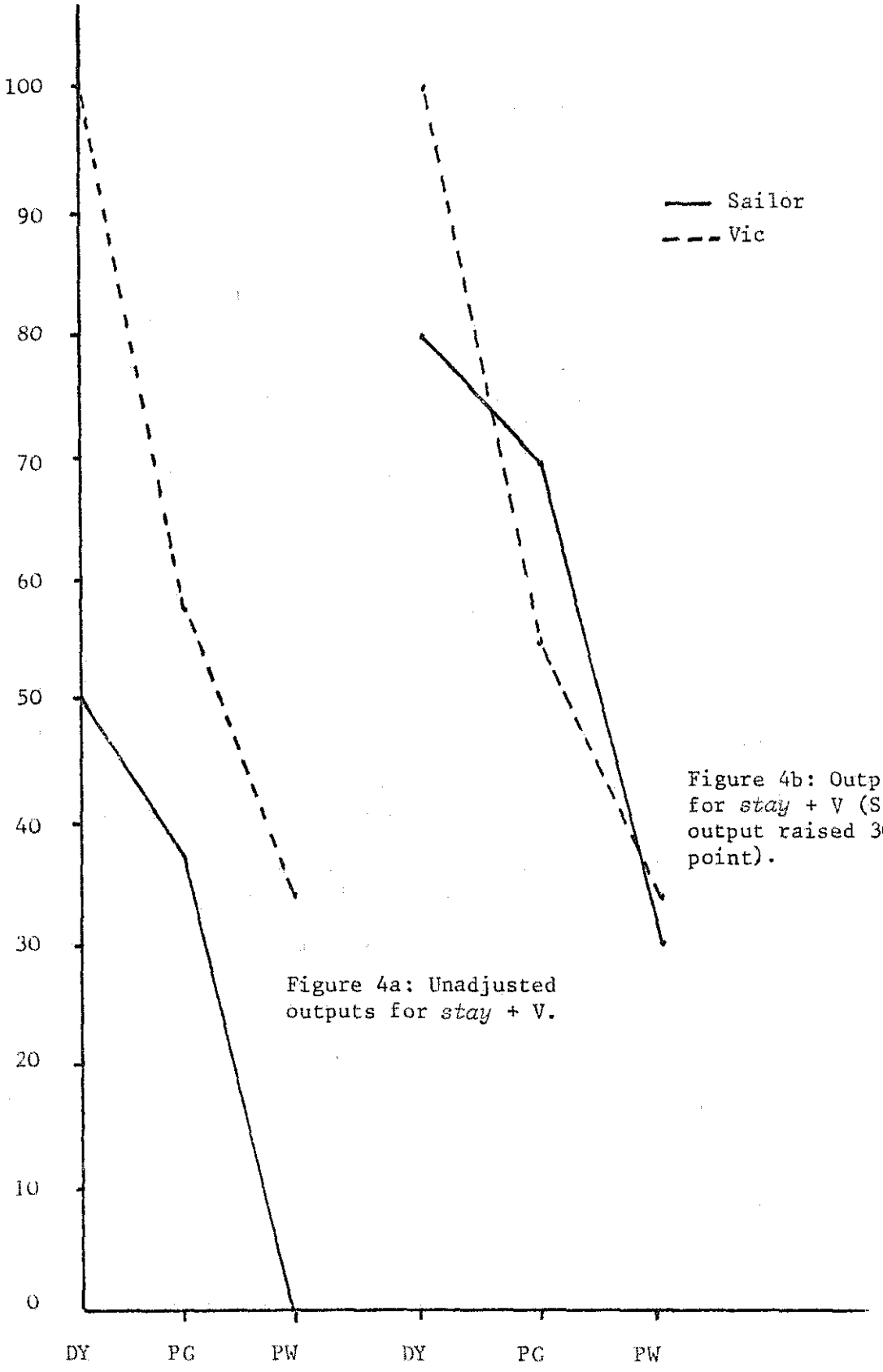


Figure 4a: Unadjusted outputs for *stay* + V.

Figure 4b: Outputs for *stay* + V (Sailor's output raised 30% point).

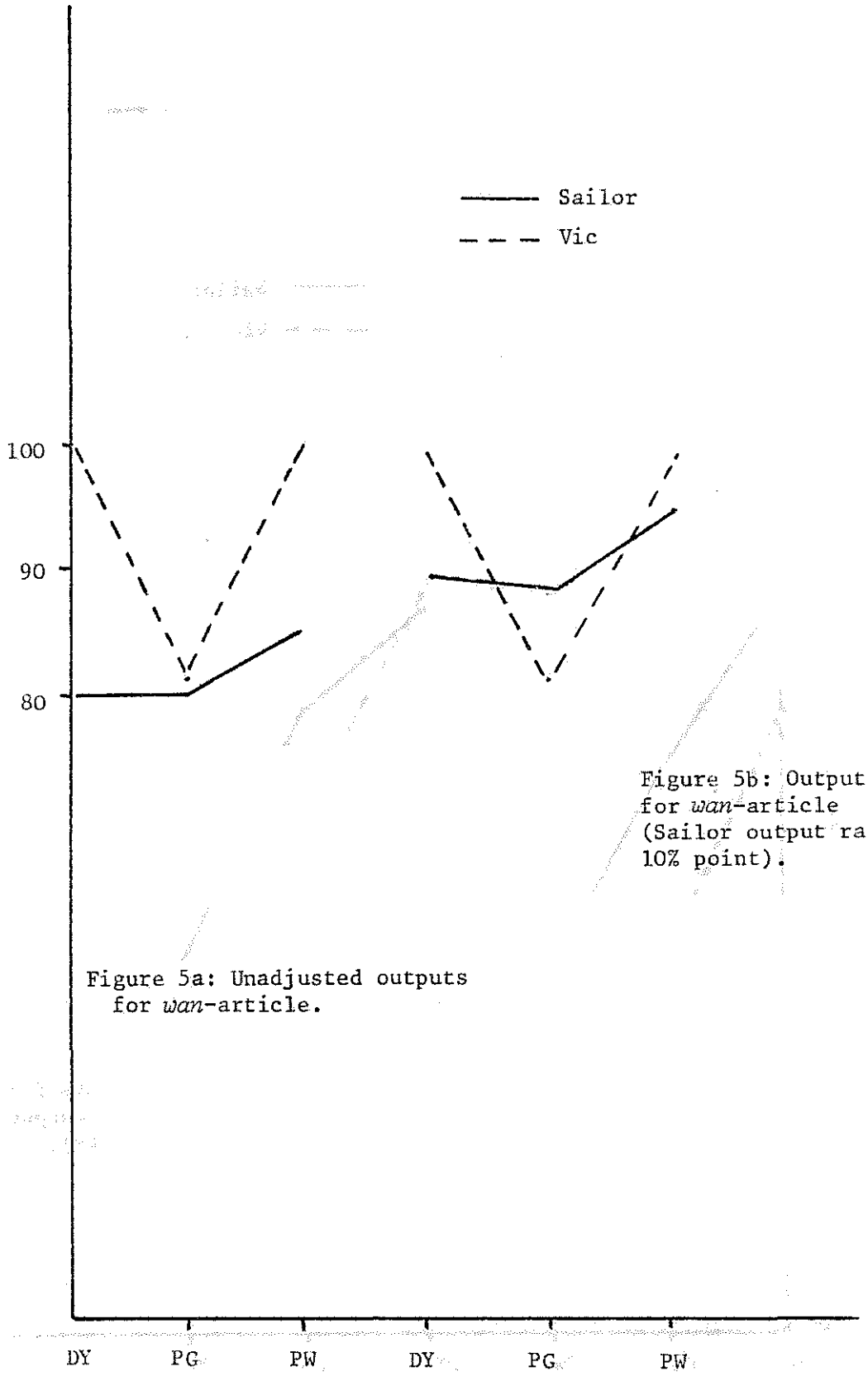


Figure 5b: Outputs for *wan*-article (Sailor output raised 10% point).

Figure 5a: Unadjusted outputs for *wan*-article.

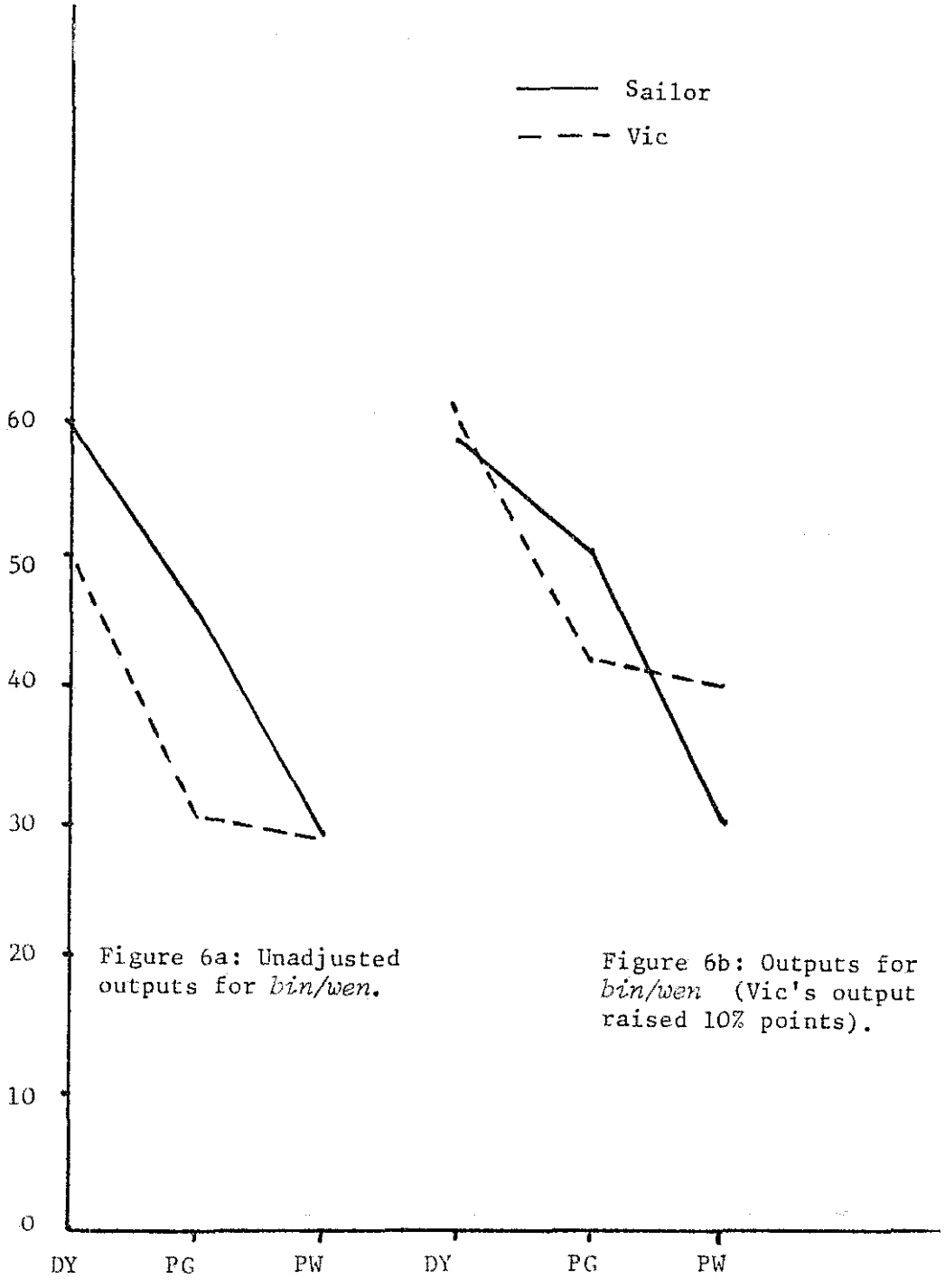


Figure 6a: Unadjusted outputs for *bin/wen*.

Figure 6b: Outputs for *bin/wen* (Vic's output raised 10% points).

role-differentiation is a natural process which will find expression through the manipulation of variable frequencies where more formal channels are lacking.

Such a conclusion would have at least one odd result. Where there are overt status-linked expressions, their use is perceived by their destined hearer, who presumably reinforces it in some kind of way. Where status-differences are expressed through frequency variations that only emerge after counting processes and statistical analysis have taken place, it is hard to believe that the hearer can be aware of their expression in any similar sense. If Sailor decreases his output of non-standard variables in the DY situation as a sign of his respect for Vic, he cannot, therefore, be doing this for Vic's benefit; he can only be satisfying his own idea of what is appropriate when talking to Vic. Thus, as with most linguistic phenomena of any interest, switching turns out to be an internal rather than an external process — to have its locus, not in society, but in the mind.

The data surveyed in the present paper is too limited in scope to enforce such radical conclusions. However, it demonstrates, at the very least, that the mechanics of switching may be more complex than hitherto suggested. Future studies will have to bear in mind the possibility that many subtleties inherent in speakers' perceptions of social situations and in differentiated roles may find reflections in linguistic behavior.

FOOTNOTES

* The research on which this study is based was supported by Grants Nos GS-39748 and SOC-7514481 from the National Science Foundation, for which acknowledgement is hereby gratefully made.

1. Especially in Bickerton and Odo, 1976: 8-12.

2. To call *bin/wen* a past marker is really an oversimplification of the facts. For basilectal speakers of Hawaiian English, it is an anterior marker, like *bin* in Guyana (see Bickerton, 1975: esp 46-47). In the course of decreolization *bin* (now almost invariably taking the surface form *wen*) evolves into a marker of simple past, as happened with *did* in Guyana (Bickerton, 1975: 69-73, 121-22). These similarities are highly significant, but lie outside the scope of the present paper.

3. Although *stay* + V is usually described in the literature (eg Carr, 1972) as a progressive form, for the most basilectal speakers it functions as a nonpunctual marker in the same way as *a* does in Guyana (Bickerton, 1975: 33-35). In the course of decreolization, it narrows to a purely progressive meaning (just as *a* does, see Bickerton, 1975: 61-69) and is the first of the Hawaiian English nonstandard tense-aspect markers to be lost completely — though *stay* remains as a locative verb for a large majority of speakers.

REFERENCES

Bickerton, D
1973 'On the nature of a creole continuum', Language 49: 640-69.

1975 Dynamics of a Creole System, Cambridge: U.P.

Bickerton, D and C Odo
1976 General Phonology and Pidgin Syntax. Vol 1 of Final Report on NSF Grant No GS-39748, University of Hawaii (mimeo).

Carr, E B
1972 Da Kine Talk, Honolulu: Hawaii U.P.

Labov, W
1966 The Social Stratification of English in New York City, Washington DC: Center for Applied Linguistics.

1969 'Contraction, deletion and inherent variability of the English copula', Language 45: 715-62.

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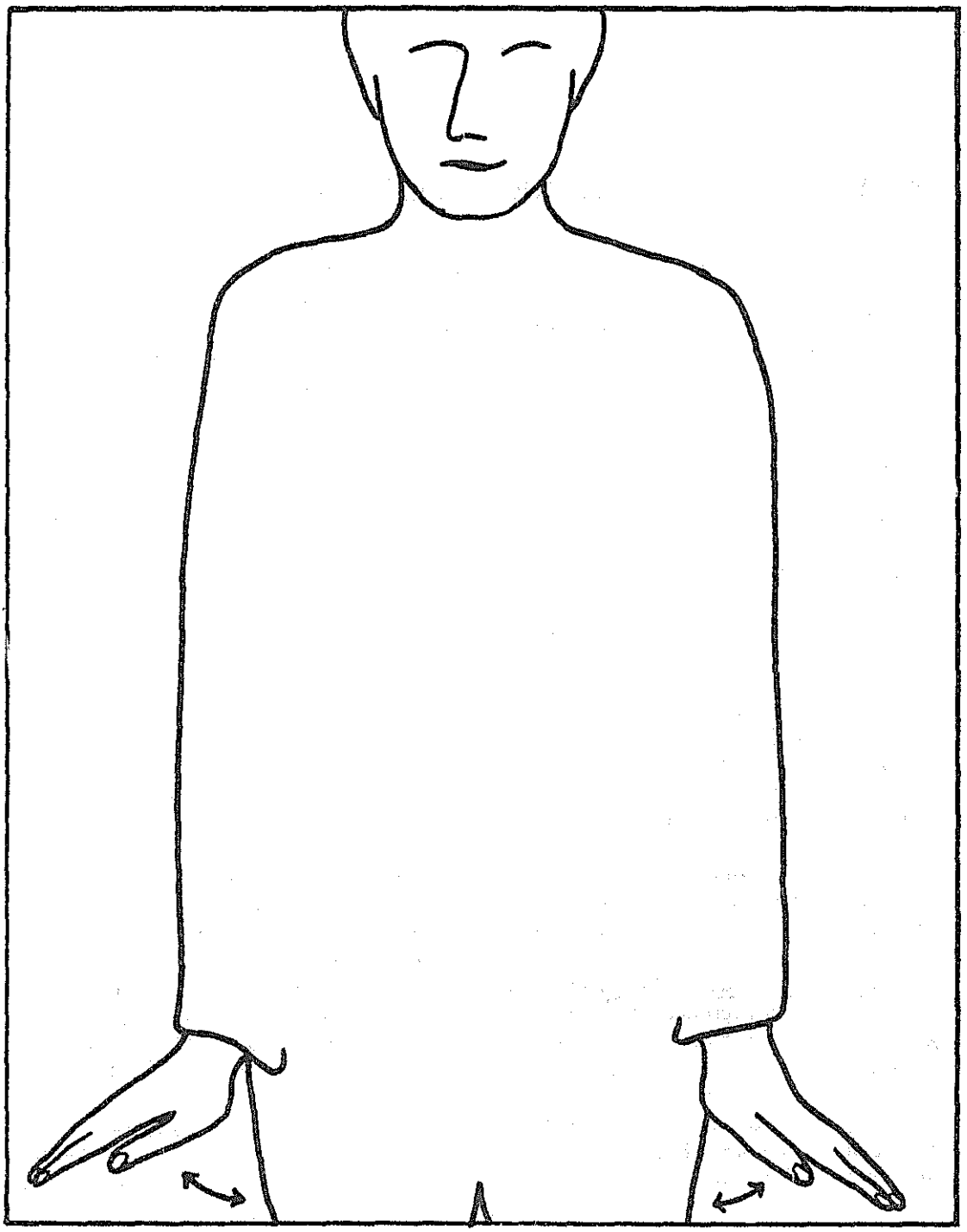
WHY ARE SIGNED LANGUAGES EASIER TO LEARN THAN SPOKEN LANGUAGES?*

The title is provocative, perhaps provoking, because it seems to presuppose something that has not been shown to be true and is likely to be false: that signed languages actually *are* easier to learn than spoken languages. If 'learning a language' is taken to mean acquiring full adult control then it certainly has not been shown that signed languages are easier to learn than spoken languages, and there are many adult would-be learners of a sign language who testify that the task is about as difficult as learning any sort of language. But, I do not have full adult control in mind. The presupposition I do intend is a more modest one. I think that the first step in language learning must be easier with signs than with words.

By the first step, I have in mind the building of a small lexicon, a set of 100 to 400 units that can be produced and can be, at least roughly, understood. I believe the first 400 or so signs must be more easily learned than the first 400 or so words. And I believe that this should be the case primarily because the first signs will, most of them, be iconic - they will, in a sense to be defined, resemble or otherwise suggest their referents - whereas the first words will not.

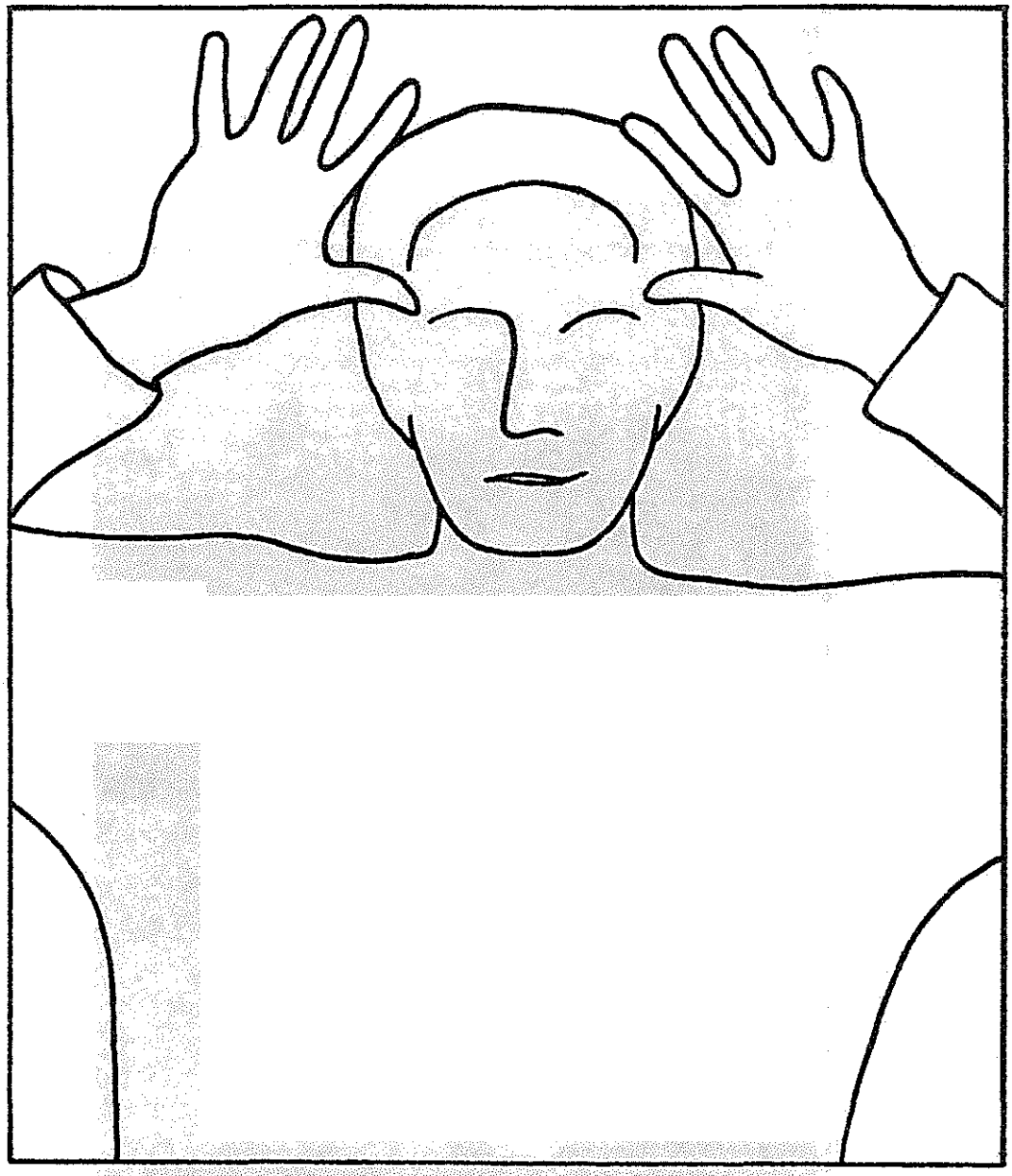
To the veteran scholar of sign languages, this will sound like the stalest of misconceptions, the surest hallmark of amateur standing, an idea long since discredited. The amateur, looking at his first pictures of signs might happen upon Figures 1 and 2 from the New South Wales Dictionary and Figure 3 from the sign language invented by Kangobai who was the first deaf person born in 24 generations on the Polynesian island of Rennell (Kuschel, 1973). To this amateur, my thesis will seem so obviously true that he hardly can believe there are any to dispute it. To me, an amateur of sign language but a veteran of some linguistic studies, not to mention World War II, it seems that the amateur is right, providing he makes a few dozen qualifications to meet well-founded objections

When the sophisticated linguistic study of ASL began, with William Stokoe's historic 1960 paper, I suppose the first question on the minds of most of us was whether ASL was a language at all, whether it had the powers and complexities of the spoken languages we knew. Stokoe's analysis made it apparent that ASL had signing or 'cherological' parallels for phonemes (with the interesting difference that they combined simultaneously rather than in linear order), parallels for morphemes, morphology, and syntax. The analysis of signs into 'cheremes' had, however, an unsought side



PENGUIN: Hold both arms straight beside body, turn hands out parallel to floor. Alternately move arms forward and back slightly.

Figure 1. The New South Wales Sign PENGUIN



DEER: Open both hands, fingers apart, palms forward. Place tips of thumbs on either side of forehead.

Figure 2. The New South Wales Sign DEER.

Figure 3. Kangobai's sign DRINK (Kuschel, 1973).

effect. When a sign is fractured into the parameters location, handshape, movement and orientation, the iconicity that might be seen in the whole often disappears. While the submorphemic analysis obviously was not done to achieve this result, the result cannot have been unwelcome to the friends of ASL. For it was Saussure's first principle of the nature of the linguistic sign that it is 'arbitrary' (1959) or 'unmotivated, ... actually has no natural connection with the signified' (p 69). And this was a general assumption in linguistic science.

As we all know, after ASL had been accorded full membership in the family of languages, Allen and Beatrice Gardner (1969) had the good idea of trying to teach it to a young chimpanzee - Washoe - and they had some success in teaching lexical items, at least. Far more success, significantly, than any of the attempts to teach chimpanzees to speak English words. Several other chimpanzees have had considerable success with ASL signs, and psychologists have been inspired to try to teach signs to children diagnosed as 'autistic' (Creedon, 1973; Miller and Miller, 1973; Bonvillian and Nelson, 1976, and in press) and also to mute, retarded children (Berger, 1972; Bricker, 1972; Wilson, 1974). The results have, of course, varied, but the following statement is safe: some chimpanzees, some autistic children, and some retarded children have learned to make and understand substantial numbers of signs when they or their like had previously failed to learn more than a very few, and often no, spoken words. This new situation has subtly changed the linguistic picture with regard to ASL and has done so in an ironic way. An axiom almost as basic in linguistics as is the arbitrariness of signs is the claim that no one language is any easier to learn than any other - as a first language. But, on the present evidence, ASL is easier in the first stages, when the teaching is deliberate, than is English for non-hearing persons, autistic and retarded children, and chimpanzees.

The irony is not a serious one, of course. Today I think most of us would agree that a communication system is a language so long as it seems fully to meet the cognitive, expressive, and communicative needs of a human community, and ASL meets that test. This simple definition of language relieves ASL and all communication systems of any obligation to match the 10 to 20 so-called 'design features' of known spoken languages and leaves ASL, and perhaps languages yet to be discovered, quite free to have idiosyncratic properties of importance - like 'cheremic' simultaneity and, perhaps, iconicity.

So long as it was only chimpanzees, in fact only Washoe, who had passed ASL and failed English, there was an obvious and appealing explanation. ASL was easier for chimpanzees because it was manual. Chimpanzees could learn it when they could not learn to speak because they had great manual dexterity and a high spontaneous performance level, whereas their articulational apparatus was greatly different from the human and was also little exercised. But see how the theoretical picture changes when we add on autistic

and retarded children. They have the same articulational organs as the rest of the human species, and it now appears that in ASL *and perhaps all signed languages the property of manual response* is just another variable, one that is perfectly confounded with iconicity, simultaneity, the 'molding' technique of training, and other properties. After all, if the manual aspect of ASL is a sufficient explanation for the success of chimpanzees, it is fair to ask why no one has undertaken to teach a member of this species to spell English with a manual alphabet. If a chimpanzee succeeded, that would put an end to all dispute as to whether the system learned was really a language or not. But, for some reason, the very idea of such an experiment seems absurd.

I have looked closely at the full set of signs Washoe ever learned; 132 in all. Looked at the descriptions of them given by the Gardners as well as depictions in several ASL manuals (eg Riekehof, 1963). It seems to me that for about 100 of the 132 signs, there is an iconicity present that a chimpanzee might quite possibly apprehend. The most interesting property of the list is that it includes almost no signs which are iconic only to those who have penetrated our culture at some depth; signs like *year*, *afternoon* and *music*. At an early stage in thinking about this work, I happened to talk on the telephone with Allen Gardner and I asked him whether he thought iconicity had played any role in Washoe's success in learning signs. He was very sceptical and pointed out that we could hardly say, at present, what is iconic for a chimp.

The most impressive success thus far in teaching ASL to an autistic child has been achieved by John Bonvillian and Keith Nelson. Their first report (1976) covered six months' training of a nine-year-old mute boy, Ted, who, by the end of the period, could produce and identify the referents of 56 signs. Their most recent report, still in press, conveys the exciting news that Ted has learned about 400 signs and is spontaneously creating novel sentences at a good rate. The first 56 signs learned by Ted have been listed in Bonvillian and Nelson (1976), and having guessed at what might be iconic for a chimpanzee, I naturally did not hesitate to do the same for an autistic nine-year-old American boy. It is my impression that about 38 of the 56 signs are obvious icons and that Ted might have noticed the resemblances involved. I wrote to John Bonvillian and Keith Nelson describing briefly my thoughts about iconicity in signs. They very kindly replied promptly and helpfully. Here are a few quotations from their joint letter. '... We would not attribute the apparent ease for handicapped individuals primarily to iconicity.' '... some of John's informal observations both with handicapped children and a brain-damaged adult would suggest some facilitative effect.' 'Our impression ... is that having a clear tie between a sign and its referent may be especially important in the early stages of learning ....' 'To a much larger extent than iconicity, we believe that it is the *visual and motor aspect* ... and the ease of molding that accounts for the early phases of sign language's success ....' 'It is quite likely that

neurological dysfunctions lie behind some such disabilities, and that visuomotor processing may sometimes bypass the disability.' A letter full of good ideas, one of which may be that iconicity is not very important.

I do recognize that my role-playing Washoe and Ted is not evidence and thatthe investigators who did these pioneering studies are very much more likely to be able to play the roles accurately than am I. I only want to record my impression, new to me if to no one else, that the signs actually learned are, most of them, signs that utilize resemblances or associations that I think the subjects might have been able to utilize. But so far, we are all guessing. The important thing is that we do not have to keep guessing, and experiments are possible that will disentangle iconicity, vision, and molding and determine what roles these facets of ASL play in learning.

One more thing for which I am indebted to Bonvillian and Nelson is a good sense of what claims I might seem to be making which I do not intend to make. I do not claim that members of the populations in question are unable to learn *any* arbitrary, unmotivated sign-referent pairs. They have all done so; therefore, they can. I take no position on how deeply members of the populations under discussion may already have penetrated into the complexities of ASL. How many signs, how much grammar, fully-reported data and careful analysis will tell. Most importantly, nothing that I will say is intended to imply that signed languages are, in any way, inferior to spoken. On the contrary, as you will see, I am in that early state of rapture which cannot tolerate any reservations at all about its object.

My simple hypothesis is that iconic signs, when the iconicity can be recognized, will be more easily learned and remembered than arbitrary signs. It appears that this cannot be taken for granted and so I have gone to the extreme lengths of performing an experiment. Not with chimpanzees or autistic children, but with twelve hearing children between the ages of four and five years who had no prior knowledge of ASL. With six of these children, an effort was made, working with one child at a time, to teach standard ASL signs translating the words CHAIR, PIANO, PEACH, SOCKS, BALL, CAR, SAW, and TURTLE. With the remaining six children, an effort was made to teach eight genuine ASL signs, but with the signs being arbitrarily and falsely assigned the same set of eight English words. Understanding of the words was in all cases pretested, using a picture book. Training always meant first demonstrating the sign, then asking the child to imitate it and molding his or her hands as necessary, and, finally, asking for a spontaneous performance. After a child had been trained on all eight signs, there was a 15-minute diversionary period, usually filled by looking at 'ABC' picture books. Then came the test on memory for the signs. It was the same for both groups of children. None was asked to make a sign, but only to recognize each sign as it was

modelled by the experimenter. This meant for six children giving the correct English translations, and it meant for the six other children speaking the same words, but in response to signs with which they had been arbitrarily linked.

Table 1

Average Numbers of Signs Learned in Two Experiments

Experiment I: ASL Signs Correctly Linked with English Words Naming Basic Objects vs ASL Signs Arbitrarily and Incorrectly Linked with the Same English Words

	Average No. Signs Learned[a]
Signs Correctly Linked (n=6)	6.8 (out of 8)
Signs Arbitrarily Linked (n=6)	2.3 (out of 8)

[a] p about .001

Experiment II: ASL Signs Correctly Linked with Abstract English Words vs ASL Signs Arbitrarily and Incorrectly Linked with the Same Abstract English Words

	Average No. Signs Learned[b]
Signs Correctly Linked (n=6)	3.8 (out of 8)
Signs Arbitrarily Linked (n=6)	1.2 (out of 8)

[b] $p \leq .05$

The results appear as Experiment I in Table 1. I have reasons for believing that the ASL signs correctly linked with the words *chair, piano, peach,* etc were somewhat iconic. At any rate, they were far more so, with respect to the meanings involved, than were the arbitrarily linked ASL signs, which really meant 'grandmother', 'earth', 'music', 'afternoon', 'year', 'week', 'people', and 'country'. You understand that these words were never given to the children in Experiment I. In the arbitrary condition, a child might be told that the sign that really meant 'week' meant 'piano', and the AFTERNOON sign meant 'socks', and so on through randomly determined pairings. This was not grossly offensive or unbelievable as it would be to you or to me. It was rather the case that the children in the arbitrary condition hunkered down after a few examples, seemingly assimilating this instruction, to their learning of the speech lexicon - an arbitrary, no-fun business. They were strikingly different in their reception of the task from the children shown the correct and often cleverly iconic signs. These latter laughed with pleasure at the good signs (especially TURTLE), sometimes tried to anticipate the sign before it could be modelled, and sometimes gave good alternatives. One child insisted that her

sign for 'peach' - which seemed to us to represent breaking it in halves because of the pit - was better than the sign we used - brushing the cheek with the back of the hand. With iconicity, a language can be witty and inventive on the level of the morpheme; something that is not possible for a spoken language.

Experiment II is, in design, essentially the same as Experiment I. That is, six hearing children had the task of learning eight ASL signs given their correct meanings, and six other hearing children had the task of learning eight different signs incorrectly and arbitrarily assigned to the same English words. For instance, in the correct assignment group, a child would be taught the sign YEAR for English *year*, but in the incorrect and arbitrary assignment, they might be taught the sign SAW for *year*. Experiment II differs from Experiment I in that the signs and meanings come from a different part of the lexicon. The words of Experiment I fall at what cognitive psychologists (Rosch *et al* 1976) usually call the Basic Object Level where I believe iconicity in ASL should be usually high and detectable by young children. The words of Experiment II are all familiar to four-year-olds and at a superficial level understood by them; we established as much by pretest. However, the meanings of ANIMAL, CLOTHES, NIGHT, PAPER, FOOD, YEAR, WEEK, and MUSIC are abstract in the sense that they do not have physical exemplars that can be pointed at. Nevertheless, there is in ASL a conventional sign for each of these abstract meanings. I have general reasons for thinking that abstract meanings like these should usually have ASL signs that are either non-iconic or else iconic at a level of sophistication above that of the four-year-old child. Hence, they should mostly be arbitrary or non-iconic for him, and it ought to make little difference whether the signs are correctly assigned or incorrectly assigned. As you can see in the bottom half of Table 1, it makes some difference; more than I anticipated. The reason why it makes some difference is not far to seek. The abstract meanings were selected arbitrarily, without knowledge of the ASL signs expressing them. As it turned out, several of the signs, especially CLOTHES, ANIMAL, and FOOD, possessed an iconicity some four-year-olds could detect and did. Once a child detected good iconicity in Experiment II, he or she usually looked for it in all signs and made iconic guesses, frequently wrong guesses. I am reminded of the way that adults usually cope with the laboratory task of learning paired nonsense syllables. They try to invent some meaningful association to link the two together. The absolute levels of correct performance in Experiment II are well below those in Experiment I and this, I believe, is because I was correct in thinking that iconicity would be less reliably present and less detectable by four-year-olds in the signs for abstract meanings than in the signs for Basic Objects. However, it was not totally absent in the signs for abstract meanings, only less strong, and that probably means that abstractions with which four-year-olds have some familiarity will not all lack an iconicity that four-year-olds can apprehend.

A few words are necessary about the significance levels for the differences in Experiments I and II. For 11 of 12 children in Experiment I, performances were sharply polarized, with no score in the iconic group lower than six and no score in the arbitrary group higher than two. In short, the distributions were non-overlapping and almost as far apart as the design permitted. However, the twelfth subject who was assigned to the arbitrary condition did not run true to the general form, but learned seven out of eight of the arbitrary signs. It may be that he was exceptionally intelligent. Defining the experimental population in terms of chronological age alone was, of course, very risky since mental ages, and so intelligence quotients, were free to vary widely. The fact that I took this risk shows how confident I felt of the truth of the hypothesis. Had the twelfth subject not been exceptional, there would have been no need to do any statistics at all. However, he not only made a statistical test necessary, but made it difficult to find just the right test. A simple 't' test, though performed, is not strictly appropriate since the two distributions are unlike and neither is even nearly normal. A non-parametric test does not make assumptions about the distributions and so is appropriate, and I did a Mann-Whitney 'U'. The trouble with the non-parametric test is that it is less powerful than is desirable in that it only requires ordinal measurement and so does not take advantage of the size of the differences between the scores in the two groups. The best test is a non-parametric 'randomization test' which assumes interval rather than ordinal measurement. In fact, I did all three tests and as it turned out, all three significance levels were close together and near the level reported: $p = .001$.

For the data of Experiment II, it was evident from very early on that the difference would need to be tested for significance. The difference between the means is only slightly less than in Experiment I, but the variance is quite a bit higher. Once again, the three kinds of test were made and once again, the three significance levels were close together and all attained the minimal value reported: $p \leq .05$.

There is one final aspect of Experiment II which, while it cannot be confidently explained, is not totally mysterious. The group given arbitrary sign-meaning pairs in II did somewhat less well than did the corresponding group in I. Why should that have been the case? The English words used for abstract meanings (eg *music, afternoon, animal, clothes,* etc) were, I am sure, less familiar to the children, simply as words, than were the Basic Object words (eg *chair, ball, car, socks,* etc). One aspect of a good performance in either experiment must be simply remembering the total list of eight words used. This is probably easier to do with very familiar words than with less familiar words. It seems worthwhile, in this connection, to look at just the words in each experiment given as responses, but proving erroneous. Such a word might be drawn from the list of eight, but used to translate the wrong sign - a 'list word misplaced' - or it might be a word drawn from outside the experiment - an 'off list' word.

In the former case, the subject seems to have the total list in memory whereas in the latter case, the full list seems not to be clearly demarcated. In Experiment I, 35 per cent of the total (26) incorrect words were 'off list' forms whereas in Experiment II, 81 per cent of the total (36) incorrect words were 'off list' forms. It does seem, therefore, that it was more difficult to keep a separate list in memory of the eight words used when these were at an abstract level than when they were at the Basic Object Level, and this difference could have resulted from differential familiarities.

Both experiments in Table 1 show that when ASL signs are iconic in a way that a naive learner can detect, without its being pointed out to him, the sign is about three times as likely to be learned as when it is arbitrary. Table 1 also suggests that iconicity detectable by four-year-old children is stronger in one part of the lexicon - the Basic Object Level - than in another, the region of abstract meanings. I must omit the details of the experiment and ask you to take it on faith that the two experimenters, Ms Kegl in Experiment I and Ms Hays in Experiment II, were warm and intelligent tutors who followed detailed instructions and never mentioned iconicity. Some precautions were taken against experimenter bias, and everything that ought to have been randomized was randomized.

The briefest and most brilliant philosophical attack on the notions of iconicity, resemblance, and similarity is, in my opinion, Nelson Goodman's 'Seven Strictures on Similarity' (1972). However, my experimental paradigm shows that I do not have in mind any of the senses of similarity or iconicity that Goodman demolishes. I do not mean an objective iconicity such as we would look to physics to explain. I have in mind a perceived iconicity, but not a universally perceived iconicity. There may be a few signs that have a motivation all members of the human species can appreciate (as Kuschel suggests), but I have no opinion on that. The iconicity I have in mind is time-bound, culture-bound, age-bound, and, in general, experience-bound. With this idea in mind, one would think the experiments would begin with a panel of judges; adult, hearing Americans totally ignorant of ASL, who would look at models of 1000 or so signs and rate the likelihood that hearing children, four to five years old, would detect the motivation in each sign. One could then undertake to teach the signs as we have done and see if judged iconicity predicts ease of learning, as I am sure it would.

I did not use this rating procedure because the hypotheses I wanted directly to test were not quite as I have represented them. I have briefly put the hypotheses in terms of iconicity and arbitrariness because that is the underlying variable of interest and because the directly-tested hypotheses cannot be understood without some additional explanation, but it seemed important to me to set the concrete data before you at the earliest possible point.

If, now, you are willing to put up with some terms not yet carefully defined, but soon to be so, I can state the hypotheses directly tested.

Experiment I asks whether ASL signs, randomly selected from signs naming Basic Objects, will be better learned when correctly linked with their English equivalents than will ASL signs arbitrarily and incorrectly linked with the same English word. Table 1 shows that correctly linked signs for Basic Objects are better learned. I believe that this is because ASL signs for Basic Objects are likely to be iconic.

The difference between testing the hypothesis just stated and a hypothesis directly concerned with iconicity and arbitrariness appears clearly in the data for two Basic Object signs: SOCKS and CHAIR. These were included because my random procedure selected them. I am sure you will agree that they would never have been included if the criterion had been iconicity as rated by judges. In the results of the children who were taught correct signs for meanings at the Basic Object Level, there were only seven errors in all and, of these, six occurred for 'socks' or 'chair'. The remarkable fact is that if one interprets the results in Experiment I strictly in accordance with iconicity, every reasonably-iconic sign was learned by all children.

The hypothesis tested in Experiment II, like that in Experiment I, does not directly concern iconicity, but rather concerns the differential learnability of signs drawn from a particular region of the lexicon when correctly translated and when incorrectly and arbitrarily translated. The region of the lexicon is defined by the conjunction of three characteristics: (a) abstract meaning; (b) a conventional ASL sign agreed upon by our informants; (c) a correct English equivalent, which is a noun familiar to most four-year-old hearing children. From the total population of signs meeting these criteria, and it is not a large population, a random selection of eight words was made. As already mentioned, the signs for these words included CLOTHES, ANIMAL, and FOOD, signs that are somewhat iconic and are so in a way that four-year-olds might apprehend. Certainly these signs would not have been included if the criterion had been ratings for the absence of iconicity. As it turned out, these three signs accounted for 11 of the 21 correct signs in the group given correct translations. While it is clear that other signs occasionally registered as iconic with one child or another, the fact is that without the three signs mentioned, the means of the two groups would not have differed significantly.

The amount of iconicity in ASL implied by Experiments I and II may seem to contradict the results of earlier experimental studies but, in fact, it does not. The difference is one of criterion. Several investigations have studied what they call the 'transparency' of sign by asking hearing subjects to try to guess

the meanings of individual signs modelled in citation form, with no special guidance of any kind. There is some variation in the rules used for deciding whether a guess is close enough to be scored correct. Hoemann (1975) used a variety of rules and, for four hearing judges and 100 ASL flash cards, obtained estimates of transparency ranging from 10 per cent to 30 per cent. Bellugi and Klima (in press) used the simplest and also the most demanding test of transparency - the ability of a naive hearing subject exactly to guess the English equivalent from sight of the sign alone. For 81 of 90 signs, not a single subject guessed the correct meaning. In effect, then, this clear-cut method suggests no iconicity at all. However, transparency, in any of the senses studied, is not relevant to a hypothesis concerning the ease with which signs will be learned when ASL is taught as a first language.

In no first-language instruction, whether of speech or ASL, whether to a chimpanzee, a retarded child, or a normal child, is the learner ever asked or expected to be able to guess the meaning of a sign or a word in isolation. The usual way of teaching the first meaningful forms is by pointing at an instance of the class of objects named, an *egg* or a *car* or a *cookie* or whatever, and saying the appropriate name or making the appropriate sign. Clearly, when the learner looks at a car and sees a sign that resembles turning a steering wheel, he is more likely to see the motivation in the sign than if it occurred in isolation.

Bellugi and Klima made a less demanding test of iconicity which is much more relevant to sign learning. Hearing judges saw both the sign and its English equivalent and were asked to describe what they considered to be the basis of the relationship between the two. In the case of the ASL sign for 'car' and the word *car*, the subject would be expected to say something like 'turning the steering wheel'. Notice that presenting together the sign and its sense is comparable to a real language learning situation. Bellugi and Klima call this procedure a test of the 'translucence' of a sign. For more than half of the 90 signs presented, there was overall agreement on the connection between a sign and its meaning. In short, something like 50 per cent of the signs were translucent and by this criterion, which is relevant to learning, iconic.

The estimate is still somewhat lower than mine would be only because I am not concerned with the ASL lexicon as a whole, but with those portions of a lexicon that are always the first to be learned. Bellugi and Klima used only signs commonly familiar to deaf persons and translatable into English nouns. They included, however, both abstract and concrete nouns. This must mean that only some of the nouns were at the Basic Object Level, which is the level at which most nouns fall that are learned in the first few years of language acquisition. Bellugi and Klima list 13 nouns to suggest the flavour of their list and, of these, only five are at the Basic Object Level. If all had been at this level, I suspect that more than 50 per cent of the signs would have proved translucent, and so the results of this study may be consistent with my hypothesis.

What, at long last, is this Basic Object Level so often invoked, but as yet undefined? If we consider any specific referent object, for instance, a certain chair in my living room at home, the object will have in English several appropriate names. The several names classify the chair more broadly or more narrowly, and they are at different heights in a taxonomic hierarchy. In the present case of a certain chair, it can be called *furniture*, at its most abstract level; it can be called *chair* or, more narrowly, *easy chair*, and ultimately, of course, one can create a phrase to serve as a proper name for just one chair. The *white easy chair in my living room* is such a proper name. Any referent at all can be classified and named at several levels: a *dime* is also a *coin* and is *money*; *ski pants* could just be called *pants* or even *clothing*. The point of interest is that among the appropriate names for a referent, there seems always to be one that strikes us as its *real* name, as the name that classifies the referent at the right level for that referent. The *real* name is the name at the Basic Object Level and *chair* and *pants* are Basic Object terms, whereas *furniture* and *clothing* are not.

The Basic Object Level does not fall consistently at the same hierarchical level in all taxonomies. In part, the level is thought to be determined by aspects of the physical world, but it can also be defined in terms of behaviour. Eleanor Rosch and her associates (1976) at Berkeley have done just this for nine taxonomies. They investigated terms at three levels: the highest, which they call superordinate, includes terms like *furniture, clothing, tool, musical instrument*, and so on. The Basic Object Level includes such terms as *piano, drum, peach, apple, hammer, saw, chair, table, pants, socks*, and so on. The subordinate level included *grand piano* and *upright piano*; *Concord grapes* and *green seedless grapes*; *knee socks* and *ankle socks*; *kitchen chair* and *living room chair*. For every term at every level, adult hearing subjects were asked to make two major lists. First, to list all of the attributes of, for example, *chairs* peculiar to these objects. Aspects of appearance or functions performed by all and distinctive of them. Notice how much easier this is to do for *chairs* than for *furniture*. Finally, list all of the motor movements you make in connection with these objects which are peculiar to them. Compare *hammer* and *saw* with *tool* or, as users of ASL, I suggest you think of such Basic Objects as *car, bicycle, cookie*, and *egg*.

You will not now be surprised to hear that there are far more characteristic attributes and characteristic movements for terms at the Basic Object Level than at either the more abstract Superordinate Level or at the more concrete Subordinate Level. But note, characteristic attributes of objects and characteristic movements performed in connection with objects are the principal sources of iconicity in ASL and, I would suppose, any sign language. Therefore, there is a greater *potential* at the Basic Object Level for the creation of iconic signs than at any other level. Elissa Newport and Ursula Bellugi (in press) have already investigated

Table 2

Movements and Attributes of Sample Superordinates and Basic Objects

Superordinates

Term		Movements		Attributes
Furniture[1]	1	Eyes: scan		
	2	Hand: grasp		
Clothing	1	Eyes: scan	1	You wear it[2]
	2	Hand: grasp	2	Keeps you warm

Terms at the Basic Object Level

Chair (sit on)	1	Head: turn	1	Legs[3]
	2	Body: turn	2	Seat[2]
	3	Body: move	3	Back[3]
	4	Body: back position	4	Arms
	5	Knees: bend[2]	5	Comfortable
	6	Arm: extend-touch	6	Four legs[2,3]
	7	Waist: bend	7	Wood
	8	Butt: touch	8	Holds people
	9	Legs: release weight		
	10	Back torso: straighten		
		(etc)		
Pants	1	Hands: grasp	1	Legs[2,3]
	2	Arms: extend	2	Buttons
	3	Back: bend	3	Belt loops
	4	Knee: position	4	Pockets[2,3]
	5	Leg: raise, extend	5	Cloth
	6	Foot: raise, extend	6	Two legs[2,3]
	7	Arms: pull up[2,3]		
	8	Fingers: grasp, pull up, twist		
	9	Elbows: bend		
		(etc)		

[1] No sign in NSW dictionary
[2] ASL sign relies upon
[3] NSW sign relies upon

(Adapted from Rosch *et al*, 1976)

ASL forms of expression and found that for terms at the Basic Object Level there are primary ASL signs, whereas for terms at the other levels, ASL uses a variety of compounding devices. Newport and Bellugi do not comment on differential iconicity at the three levels, but from their discussion and illustrations, it is obvious that the Basic Object signs are more iconic than the others.

Table 2 shows how the link between sign iconicity and Basic Objects works - probably in all sign languages. The data are from the paper by Rosch and her associates. For the two Superordinate terms, *furniture* and *clothing*, you can see that hearing subjects could think of few or no characteristic movements and attributes. For the Basic Object terms *chair* and *pants*, they could think of many. These characteristic attributes and movements are raw material for the invention of iconic signs for *chair* and *pants*. I have marked some that appear to be used in the ASL signs or the New South Wales signs. Clearly, there is plenty of room to accommodate the fact that signs for a given object may be iconic in a number of sign languages and yet select different features to utilize.

Note, in passing, that this discussion is entirely concerned with signs having noun equivalents because the theoretical and experimental work on the Basic Object Level has, thus far, been limited to nouns. However, a similar distinction can certainly be made for verbs, and the iconicity of signs that translate verbs is probably superior to that of signs that translate nouns.

It remains to show that the Basic Object Level, where the opportunities for iconic creation are maximal, is also heavily over-represented in the lexicons of child speech. Carolyn Mervis (Rosch *et al*, 1976) borrowed from us all of the 31 weekly speech samples belonging to Stage I of the hearing child, Sarah, who is one of three we have studied. Mervis extracted from the lexicons all occurrences of any term from any of the nine taxonomies for which the Basic Object Level had been determined. Practically every noun in Sarah's lexicon at Stage I was at the Basic Object Level, and I have found the same to be true for the other two children we have studied, Adam and Eve.

In addition, I have found that lexical growth, with respect to nouns, for all three children in all major taxonomies is, for the first five stages of development, almost entirely at the Basic Object Level.

The thesis may be summarized as follows. At the Basic Object Level, referents have a maximal number of distinctive attributes and movements. Distinctive attributes and movements constitute the raw material of iconic signs. Therefore, there is a maximal potential for iconicity at the Basic Object Level. When a random assortment of signs from this level is learned by naive hearing subjects either correctly linked with referents or arbitrarily linked, the correctly linked signs are learned much more easily than the arbitrarily linked. Presumably, this is because of a

contrast in iconicity, and when terms are randomly drawn from among abstract terms, rather than Basic Object Level terms, there is less contrast in ease of learning. Presumably, this is because there is *less* contrast of iconicity. Since nouns at the Basic Object Level constitute the great preponderance of all nouns in the early stages of learning a first language, and since signs at this level can be and are, usually, iconic, whereas words are not and cannot be, and iconicity makes learning easier, signed languages are easier to learn, at first, than are spoken languages.

In putting forward some claims for iconicity, I do not mean to challenge the proven importance of the formal properties of ASL. At several points, what at first appears to be a conflict resolves into a mutual clarification. Bellugi, Klima and Siple have done a beautiful set of experiments which they call 'Remembering in Signs' (1974-75). One discovery that does seem to leave little room for iconicism concerns the manner of encoding linguistic material in short-term memory. For hearing persons, this study, with many others, shows that verbal material is encoded in phonological rather than visual or semantic terms. One works this out chiefly from what are called intrusion errors: *vote* is mis-remembered as *boat*; *horse* as *house*. How, the authors ask, do non-hearing persons, for whom ASL is the first language, encode signs in short-term memory?

Their data point unequivocally to storage in terms of the cherological equivalents of phonology. In particular, the authors write that there are *no errors at all* that would suggest iconic coding. The sign for *cat*, for instance, was never mis-remembered as 'whiskers' or 'fur' or anything like that. Hard lines for iconicity.

What the data do show is '... the majority of the multiple errors preserved all but one of the parameters of the original sign and differed from the sign presented in only one prime of one parameter' (pp 112-13). Thus, SOCKS was mis-remembered as STAR, a change of orientation alone; and HOME as YESTERDAY, a change of configuration alone. The memory involved in our sign-learning task is not short-term, but that need not matter. The main question is, how did the children in Experiment I, who learned correct iconic signs for either Basic Objects or abstract meanings, encode those signs in memory? If not iconically or visually, then it is doubtful that the iconicity present should be given the credit for their rapid learning.

The children learning the correct signs for Basic Objects did so well that there were not many errors of any kind, let alone intrusion errors. However, all the errors they made were conceivably iconic, though some might also be considered formational. The sign for *socks* was twice remembered as *saw*. There was a similarity of movement in the signs we used. The sign for *chair* was mis-remembered as *turtle*; in both cases something is placed on something else. Finally, the sign for *ball* was once mis-remembered as *peach*.

It was the children in Experiment II, who were taught correct signs for abstract meanings, who provided the richest collection of iconic intrusions. What often happened was that the child would catch the 'motivation' of one or more of the signs (most commonly FOOD, ANIMAL, or CLOTHES) and would then guess at the meaning of almost every sign, usually getting it wrong, but always for iconic reasons. Thus, the FOOD sign was mis-remembered as 'cracker', 'ice cream', and 'eat'; all of them excellent iconic intrusions. The MUSIC sign was called 'rocking a baby', ANIMAL was 'Teddy Bear' and 'monkey' and 'scratch', and MORNING was 'up'. In all, there were 12 iconic intrusions in this group, which is certainly sufficient grounds for arguing iconic coding in memory.

I think the errors we obtained suggest that the newcomer to ASL may encode signs iconically, but I am not at all surprised that fluent adult native signers do not. With a large lexicon to encode distinctively and with the need to process ASL speedily, there can be neither space nor time for iconicity. Perhaps this is the real reason why an infatuation with the iconic face of sign vaguely suggests an amateur state of mind, while the formal face is definitely turned toward the expert. I even fancy that the iconic signs I know, and know because of their iconicity, have already begun to fade into dead metaphors.

Finally, I must certainly find a response to Nancy Frishberg's conclusion about historical change in signs: 'In general, when signs change, they tend strongly to change away from their imitative origins in pantomimic or iconic gestures toward more arbitrary and conventional signs' (in press, p 6). If iconicity has any significant social value, it ought not to be possible for it to disappear altogether, and my impression from Frishberg's work is that she would not expect this.

During the same years in which iconicity is judged to have been declining in ASL, Frishberg reports that its morpheme structure constraints have tightened. There is more focus on the hands and less use of the body and face; sign locations come to be limited to the clearly circumscribed signing space; there is a trend toward symmetry in the articulators, and so forth. One way to think of the formal properties of ASL and of the iconic properties is as two opposed principles, one of which must completely prevail in the end. There are other possibilities. Perhaps the two interact in a sign language in such a way that maximal expressiveness, learnability, and productivity depend upon a certain balance rather than a total victory of either.

I think historical change in writing systems which, so far as I know, has always been from representation to arbitrariness may influence us when we think about change in signs, but the two cases are very unlike. For one thing, writing systems, in sacrificing representation, were able to utilize the vastly more powerful economy of the alphabetic priniciple. For another, signing

communities seem usually, perhaps always, to exist in the midst of larger speaking communities, and the smaller group has some need from time to time to transmit simple messages to the larger. And what will help more than iconicity? Ask Kangobai of Rennell. Finally, sign languages, like all languages, break up into dialects. But when users of different dialects gather together, I am told that they fairly quickly find a way to make themselves understood and that this wonderful process takes off from a platform of maximally iconic signs.

A few years ago, a Harvard undergraduate wrote an honours thesis with me on French proverbs in, I think, the seventeenth and eighteenth centuries. He was enormously ingenious at identifying the many constraints of the form: patterns of rhyme and alliteration and stress; length of line and partial repetitions. At first, we thought of all this as so much ornament, designed to trick out commonplace thoughts in finery. Eventually, it occurred to us that this common assumption was probably false. The constraints on French proverbs were there in order to provide an opportunity for the exercise of wit. Without the constraints, there is no problem in writing a proverb, except that there is also nothing you would want to call a proverb any more.

I suggest that the formal properties of ASL, in addition to whatever their advantages may be in terms of information processing, also provide an opportunity for the exercise of wit, iconic wit. The iconic signs are not just representational or suggestive; they are iconic in a very economic way and within the rules of a very tight game. If the first 400 signs of ASL were, in fact, 400 life-size, full-colour photographs of Basic Objects, they would be a whale of a lot more iconic than the little tricks we do with our fingers. But I do not think that either child or adult would laugh aloud as many now do at the cleverness of the signs for *turtle* and *snail* and *cookie* and *bicycle*. Perhaps the historical changes seen in the past 50 years in ASL will not, in the long run, prove to be the first segment of a long straight line, but rather one phase in a cycle around a point of balance. Whatever happens in the future, we newcomers to ASL today find peculiarly exhilarating a language that is witty at the morphemic level as is no spoken language.

FOOTNOTE

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REFERENCES

Bellugi, U and E S Klima
in press 'Two faces of sign: Iconic and abstract', in E S Klima and U Bellugi (ed.) The Signs of Language, Cambridge, Mass: Harvard U.P.

Bellugi, U, E S Klima and P Siple
1974-75 'Remembering in signs', Cognition; International Journal of Cognitive Psychology 3.2: 93-125.

Berger, S L
1972 'A clinical program for developing multi-modal language responses with atypical deaf children', in J E McLean, D E Yoder and R L Schiefelbusch (ed.) Language Intervention with the Retarded, Baltimore: University Park Press, 212-35.

Bode, L
1974 'Communication of agent, object, and indirect object in signed and spoken languages', Perceptual and Motor Skills 39: 1151-58.

Bonvillian, J D and K E Nelson
1976 'Sign language acquisition in a mute autistic boy', Journal of Speech and Hearing Disorders 41.3: 339-47.

1977 Personal communication to R Brown, Harvard University.

in press 'Development of sign language in autistic children and other language-handicapped individuals', in P Siple (ed.) Understanding Language through Sign Language Research, New York: Academic Press.

Bonvillian, J D, K E Nelson and V R Charrow
1978 'Languages and language-related skills in deaf and hearing children', Sign Language Studies 12: 211-48.

Breger, I
1970 'Perception of sign language of the deaf', Perceptual and Motor Skills 31: 426.

Bricker, D D
1972 'Imitative sign training as a facilitator of word-object association with low-functioning children', American Journal of Mental Deficiency 76: 509-16.

Charteris, L
1972 Paleneo: A Universal Sign Language, London: Hodder and Stoughton.

Creedon, M P
1973 'Language development in non-verbal autistic children using a simultaneous communication system'. Paper presented at the biennial meeting of the SRCD, Philadelphia.

Fischer, S
1975 'Influences on word order change in American Sign Language', in C N Li (ed.) Word Order and Word Order Change, Austin: Texas U.P., 3-25.

Frishberg, N
in press 'Historical change in signs', in E S Klima and U Bellugi (ed.) The Signs of Language, Cambridge, Mass: Harvard U.P.

Gardner, B T and R A Gardner
1975 'Evidence for sentence constituents in the early utterances of child and chimpanzee', Journal of Experimental Psychology 104.3: 244-67.

Gardner, R A and B T Gardner
1969 'Teaching sign language to a chimpanzee', Science 165: 664-72.

Goodman, N
1972 'Seven strictures on similarity', in N Goodman, Problems and Projects, New York: Bobbs-Merrill, 437-47.

Hagerty, C
1976 American Sign Language Dictionary, El Cerrito, Ca: American Sign Language Society.

Hoemann, H W
1975 'The transparency of meaning in sign language gestures', Sign Language Studies 6-9: 151.

Hoemann, H and S Hoemann
1973 Sign Language Flash Cards. Published by National Association of the Deaf.

Huffman, J, B Hoffman, D Gransee, A Fox, J James and J Schmitz
1975 Sign Language for Everyone, Northridge, Ca: Joyce Motion Picture Company.

Jordan, I K
1975 'A referential communication study of signers and speakers using linguistic reference', Sign Language Studies 6: 64-79.

Kuschel, R
1973 'The silent inventor: The creation of a sign language by the only deaf-mute on a Polynesian island', Sign Language Studies 3: 1-28.

Mandel, M
1975 Iconic devices in American Sign Language. Dissertation, University of California, Berkeley.

Miller, A and E E Miller
1973 'Cognitive developmental training with elevated boards and sign language', Journal of Autism and Child Schizophrenia 3: 65-85.

Nash, J E
1973 'Cues or signs: A case study in language acquisition', Sign Language Studies 3: 79-92.

National Association of the Deaf
A Basic Course in Manual Communication

Newport, E L and U Bellugi
in press 'Linguistic expression of category levels in ASL', in E S Klima and U Bellugi (ed.) The Signs of Language, Cambridge, Mass: Harvard U.P.

Riekehof, L
1963 Talk to the Deaf, Springfield, Mo: Gospel Publishing House.

Rosch, E, C B Mervis, W Gray, D Johnson and P Boyes-Braem
1976 'Basic objects in natural categories', Cognitive Psychology 8: 382-439.

Saussure, F de
1959 Course in General Linguistics, New York: Philosophical Library.

Schlesinger, H S and K P Meadow
1972 Sound and Sign: Childhood Deafness and Mental Health, Berkeley: California U.P.

Smith, G, A Kentwell and R Dyson
Let's Talk with Our Hands; NSW Dictionary and Signs for Communication with Deaf Children, NSW, Australia: North Rocks Central School for Deaf Children.

Steklis, H D and S R Harnad
1976 'From hand to mouth: Some critical stages in the evolution of language', in S R Harnad, H D Steklis and J Lancaster (ed.) Origins and Evolution of Language and Speech, New York: Annals of the New York Academy of Sciences 280.

Stokoe, W C, Jr
1960 'Sign language structure: An outline of the visual communication systems of the American Deaf', Studies in Linguistics, Occasional Papers 8.

Wilson, P S
1974 'Sign language as a means of communication for the mentally retarded'. Paper presented at the Annual Meeting of the EPA, Philadelphia.

Wundt, W
1973 'The language of gesture', in T Sebeok (ed.) Approaches to Semiotics, The Hague: Mouton

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PHONOSYMBOLISM - SOME NOTES

In the high old times of structural linguistics, when the *-eme* was the new key to scientific language study, its proponents were nevertheless not unaware that it did not answer all questions. Units are essential to measurement or any kind of orderly handling, but sub-unitary features not easily measurable nevertheless exist and have a communicative function. At the end of Chapter 14 of Language, Leonard Bloomfield listed a series of English 'symbolic' words testifying to the existence of a 'system of initial and final *root-forming morphemes* of vague signification', such as

[fl-] 'moving light': *flash, flare, flame, flick-er, flimm-er*

[fl-] 'movement in air': *fly, flap, flit (flutt-er)*

and [-ɛʃ] 'violent movement': *bash, clash, crash, dash, mash gnash, slash, splash.*

He ends the chapter with a short summary:

> The analysis of minute features, such as the root-forming morphemes, is bound to be uncertain and incomplete, because a phonetic similarity, such as, say, the [b-] in *box, beat, bang,* represents a linguistic form only when it is accompanied by a semantic similarity, and for this last, which belongs to the practical world, we have no standard of measurement.

So far as I know, Bloomfield carried the subject no further, but others have had more or less to say on the same or similar matters under such terms as phonesthetics, sound-symbolism, and (the term I prefer) phonosymbolism. Nevertheless, the subject has attracted less attention than it deserves and remains somewhat nebulous in many people's minds. In this short paper I hope only to disperse some of the clouds by making a necessary distinction, and thus perhaps to clarify terminology and sharpen the focus of interest.

The first distinction to be made is a basic one between *onomatopoeia* and what I call *phonosymbolism*. Onomatopoeia is properly limited to that class of lexemes in which the word is made or 'named' from or by imitation of sounds - such words as *quack,* a duck's call; *bang,* the noise made by a hard blow, an explosion, or the like; *drip,* the noise made by a small and intermittent fall of a liquid. These are elemental words, using direct sound-imitation; they are not symbolic. Sir James Murray, editing the noun *echo* in 1880 for the Oxford English Dictionary coined the adjective *echoic,* and appears to have used it himself in the strict sense: the human speaker echoes with his voice a sound he hears in nature, thus producing (once conventionalized into his linguistic system) a word.

Some dictionaries recognize this, if not always strictly, in their etymological label 'of imitative origin'.

Once such a word is produced, it may, like any other word, undergo semantic developments, including symbolic extensions. Thus *quacking* comes to mean stupid or querulous talk; a *bang* may symbolize sudden forcefulness; a *drip* may be a boring person. In such developed senses, the original echoism may be masked or wholly lost. Sound change may also disguise the original echoism: English *cow* has undergone normal early-modern-English diphthongization from OE *cū*, which is certainly a closer echo of the cow's noise (imitated by other speakers and in other languages as [mu:], [bu:, bø:, bo:] [lu:, lo:] etc).[1] The distinction, however, is valid and necessary between *echoism*, or direct vocal imitation of sounds heard, and *phonosymbolism*, in which the word or a part of it (such as Bloomfield's 'root-forming morpheme') bears some other relationship between the sound component and the understood meaning, which may be called 'symbolic'. The word itself is a phonosymbolic formation and is an example of the phenomenon of phonosymbolism. It is this relationship which I should like briefly to consider.

Perhaps most obvious is ablaut or vowel gradation. It was long ago observed that, for example, in Germanic 'strong' verbs such as *sing, sang, sung; befehlen, befahl, befohlen; brekke, brakk, brukket,* there is a phonetic progression from high and/or front to low and/or back. This is by no means simply regular; there are numerous exceptions or contradictions, some of which are due to sound change. Nevertheless, there would seem to be a persistent sense of pattern. Folk formations such as American English *snuck* for *sneaked* testify to this:[2] if (as is likely) *sneak* is from OE *snīcan*, 'to creep, to crawl', the ablaut series ī-ā-i-i does not support it. It does not follow any regular English sound-change nor are there any similar forms for other words of its class (*ride, rise, write,* etc) which could have furnished a direct analogical base.[3] The only clue is that it fits the front-to-back/high-to-low progression from present or timeless forms to past forms, still to some extent productive in present-day English. It is quite obvious that the general historical trend has been from strong verbs to weak; many old strong verbs (including *sneak*) have gone over, and all newly formed or adopted verbs are weak. Nevertheless, such strong verbs as survive are apparently enough to keep this pattern alive. Other folk examples of 'new' strong verb forms are, *wove* for waved, *brung* for brought, *dove* for dived. Here phonosymbolism means that an element of sound (the admittedly vague native-speaker sense of a series of contrasting sound-types) symbolizes the similar sense of time-contrast between present and past.

This sense of vowel-gradation symbolizing something other than sheer sound (as it does in echoic words) may also be seen in groups of echoic words that contrast semantically. An interesting experiment can be made with children by telling them a story and asking their explanation. Someone is dropping stones into a body of water. One stone goes *splash*, another goes *splish*, another goes *sploosh*: what makes the difference? Almost any child will see that it is

the size of the stone, or the amount of water displaced, and the quick child will rearrange the progressive sizes into the series *splish, splash, sploosh.*[4]

Phonosymbolism of this kind has surely taken a part in the semantic development of various contrasting words in English, a very few examples of which may be given here (verbs and nouns):

High/front stressed vowel	Low/back stressed vowel	Semantic contrast
niche	*notch*	Size: small/large
slit	*slot*	
flip	*flop*	Weight: light/
slip	*slop*	heavy
sweep	*swoop*	Movement in space:
skip	*scoop*	shallow/deep
skid	*scud*	Movement in time: sudden/steady
chip	*chop*	Strength or force:
grip	*gripe*	weak/strong
click	*clack*	Sound quality:
clink	*clunk*	sharp/dull

These few examples are minimal pairs, in which all other possibly functioning factors than the contrasting vowels are neutralized. But two points must be made. First, not all such phonetically minimal pairs will show any semantic association or contrast: consider *kick* vs *cook, peel* vs *pool, can* vs *coon,* and hundreds more - far more than those having a semantic association. As in etymology, both form and meaning must correspond before a valid relationship can be believed in. Second, in many non-minimal pairs or groups there may still be an effect of vocalic phonosymbolism: consider *stiff* vs *slack, bright* vs *gloomy* - even, perhaps, such a series as *twig* vs *stem* vs *branch* vs *trunk.* But such assumptions easily become dangerous and must be made with caution. When closer study of consonantal phonosymbolism has been made, however - a complex task which holds interesting possibilities for future research - better analyses of such words will become possible.

It must be insisted that the individual contrasting words may be of quite different and unrelated sources. One may have existed in the language long before the other or others were developed or borrowed. In such cases it is the existence of the phonosymbolic pattern in the native-speaker's consciousness that predisposes him to develop or borrow a new word: it fits a slot in the pattern, and to that extent seems 'more natural' or acceptable. If there was ever anything to the old 'ding-dong' theory of language origin,[5] this was its underlying idea: the *ding* calls for a corresponding and fulfilling *dong.*

A further point is that these correspondences cannot be predicted. The phonosymbolic pattern must emerge from the linguist's observation of this recurrent relationship in sufficient numbers of word pairs or groups so that *mere* coincidence is ruled out. But the assumption that because some echoic words have mates all must have them, or further, that because many do not have them the phonosymbolic pattern is disproved, would be false. It is a matter of potentiality, which is realized in only *some* of the possible cases, and unpredictably. A pattern may be productive without immediately or sometimes ever, producing all the forms it makes possible. The possibilities for new word-formation in this mode are both open and to some extent limited. Such inventions as Kipling's *schloop, squoggy,* and other words[6] are predicted on the phonosymbolic force of the vowels, as well as on direct echoism. (The *sch-* in contrast to *s-* can probably be shown to have a similar effect, but that is not our present concern.)

If it is argued that the high-front vowels in *little, teetle, tiny, teeny, eentsy-weentsy* and so on indicate small size, and that a great many other words use high-front vowels similarly, what about *big* and *small*, which reverse the contrast? The answer in this particular instance is that for *small,* which in OE had the low-front vowel [æ], a sound law has proved stronger than the phonosymbolic pattern; and that for *big,* of unknown etymology, one suspects unrounding of ME [y], and umlauted [u], and kinship with Norwegian dialectal *bugge,* a strong man, from the IE base **beu-* - thus a sound in origin relatively back, also drawn away from the phonosymbolic pattern by operation of a sound law. Once the prevailing phonosymbolic patterns of a language have been traced, they will help to clear up anomalies and fill gaps in etymology,especially for those words which do not follow the laws of regular sound change. Their application will reduce the element of surmise which has always plagued the practice of etymology.

The most striking example of phonosymbolism that I have come upon is the use of vowel contrast - again between high and/or front and low and/or back - in demonstratives. In some languages the system is not merely simple *this* vs *that*: Latin *hic*, 'this' (near me), *iste,* 'that' (near you), *ille,* 'that' (away from both me and you, yonder). In Bantu languages it is extremely elaborate, taking into account also the closeness or distantness of the interlocutors with regard to each other. But in languages generally there seems to be at least a two-way contrast between close to and away from the speaker. It is not always or only indicated by vowel contrast - other devices are used including gesture and intonation[7]- but enough languages in the world correspond in using this phonosymbolic device so that it would seem to be at least a semi-universal. A very small list of such contrasting demonstratives meaning very much the same as *this* (closer to the speaker) and *that* (farther from the speaker) is the following:

	Near the speaker	Away from the speaker
French	*ceci*	*cela*
German	*dies*	*dass*
Dutch	*dese*	*dat*
Norwegian	*ditt(en)*	*datt(en)*

	Near the speaker	Away from the speaker
Tongan	*ko eni*	*ko ena*
Hawaiian	*keia*	*'oia*
Efik	*emi*	*ðru*
Mang'anja	*ichi*	*icho*
Shona	*-u/-i/-a*	*-o*
Maya	*lelá*	*leló*
Ojibwa	*ma·pa*	*uwiti*
Hindustani	*yih, yihī*	*wuh, jo*

It is not always possible to draw sharp lines of demarcation, expecially in matters of meaning. The behaviouristic proof that a linguistic signal 'means the same' to the hearer as it meant to the speaker is that the stimulus produced the expected or intended response. This is some help, but very gross help, largely limited to the sphere of simple physical actions. What a word means outside this primitive sphere is always potentially modifiable: literal meanings easily become transferred, metaphorized, reversed through irony, or are given qualifying emotional or attitudinal shadings. The lexicographer, having to number the meanings of a word as if they were always clearly discrete, finds himself driven to choosing those illustrative examples which are more precisely separated, leaving aside those which merge or shade from one nuance to another. He faces the dilemma of the numerating analyst who must find seven colours in the rainbow or eight semitones in the chromatic musical octave: the divisions are artificially introduced (since they are not in nature) for convenience in dealing with them at all.

There is no guarantee whatever that the complex of 'properties' that compose a speaker's understanding of what a word means to him - what he would use it to mean - and the complex of 'properties' that compose the hearer's understanding of what the work means to him will correspond in every detail. There will be a core or cluster of the same properties, a central area of congruence, but the surrounding area will show more or less non-congruence or even incongruity. In English, the effects of both echoism and phonosymbolism with various formal devices and for individual participating words, will vary from being rather central, as in the case of the demonstratives, to less so in the strong verb gradation, to peripheral or only partly active in the case of the echoic pairs. That phonosymbolism is a linguistic reality, however, can hardly be doubted. We need to examine many languages, lexeme by lexeme, and even phoneme by phoneme, to find and fully describe the places and the ways in which it functions.

FOOTNOTES

1. An interesting distinction is made in French: cows go [mø], bulls go [bø]. *La petite différence.*

2. The earliest example found in print is from 1890. See *Dialect Notes* 1.62.

3. *Druv*, for drove, is too rare to qualify.

4. The experiment should be tried with children speaking other languages. Voice volume and level should be kept the same for all words, and gestures should be omitted.

5. Once maintained, later rejected by Max Müller.

6. Just So Stories (The Elephant Child) 'He schlooped up a schloop of mud ... and slapped it on his head, where it made a cool schloopy-sloshy mud-cap.' Also later reference to 'squoggy, marshy country somewhere in Africa'.

7. The same word is often used for both close and not-close referents in cases where the element of contrast is subordinated, or where the referent is indicated by a gesture. Or, as in Japanese, the difference is signaled by consonant variation: *kore* (thing close to speaker), *fore* (close to hearer), *are* (away from both speaker and hearer).

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LANGUAGE IDENTITY, LANGUAGE FUNCTIONS AND SOCIAL STATUS*

Language is not only a means of social communication but also a potent and rich source of symbolic significance. As an identity symbol, it can act as a cementing force and unite diverse social groups geographically separated into a single cohesive organization, or divide a population into socially and politically recognized separate speech communities or nation-states. The symbolic significance of language as a strong identity token is interdependent with such processes as social differentiation, social mobilization and political modernization manifest in varying degrees in the speech community. Similarly, the significance of language as a mark of social status and prestige depends not only on the social functions performed by the language, but also on the socio-cultural organization of the community. In the present study three aspects of the symbolic significance of language for group identity, social status and instrumental function have been explored from the point of view of Dakkhini Urdu speakers with respect to the use of Dakkhini Urdu, Standard Urdu and Kannada. It presents an analysis and interpretation of a part of the data on the project "Language Use, Language Attitudes and Language Identity among Linguistic Minorities: A Case Study of Dakkhini Urdu Speakers in Mysore." The first section of the paper provides background information to the project and the data being analysed here, and the second and third sections deal with the analysis and interpretation of the data.

The Urdu speakers in the country constitute 5.22% of the total population of India according to Census 1971. They occupy sixth rank in terms of total speakers among the schedule VIII languages. In all the states and union territories of India they vary in size and strength from region to region and constitute a linguistic minority with respect to the other languages of the region. In Karnataka, for instance, 8.99% of the total population are Urdu speakers.

The question of Urdu has been entangled with religion (Brass, 1975). Not all Urdu speakers are Muslims. This goes against identifying Muslims as an Urdu speaking linguistic minority. On the other hand, not all Muslims are Urdu speakers. 46.57% of the total Muslim population in the country report Urdu as their mother tongue. The percentage of Muslims who report Urdu as their mother tongue varies from region to region. In Karnataka, for instance, 84.70% of the total Muslim population in the state are Urdu speakers. Thus, the mobilization of Muslims as Urdu speakers and the recognition of Urdu as the language exclusively of Muslims are two aspects of the process of group identity and social solidarity. The identification of Urdu with Muslims shows that 'the recognition of language boundaries, the interpretation of language boundaries and the manipulation of language boundaries are all ethnically encumberal behaviours as are *either the acceptance or rejection of whatever language boundaries* that may "exist" at any particular time' (Fishman, 1977).

In Mysore city the total Muslim population is 70526 which constitutes 19.9% of the total population of the city. The Muslims speak a variety of Urdu known as Dakkhini Urdu as their mother tongue, mostly at home and in friendship and other close networks of communication. The Dakkhini Urdu shows phonological and grammatical features distinct from Standard Urdu, and may be further distinguished into several local varieties on the basis of whether it is spoken in Karnataka, Andhra Pradesh or Tamilnadu. Thus, it may serve as an identity symbol at the local or regional level. The Dakkhini Urdu speakers also have competence in Standard Urdu in varying degrees depending upon social, educational and other factors. The Standard Urdu is used for communication in formal settings and cultural activities like mushaira,and in education and literature. Thus, it serves as a symbol of supra-local identity with Standard Urdu speakers in other parts of the country. The Dakkhini Urdu speakers also use Kannada, the dominant language of Karnataka, in interacting with native Kannada speakers, and show quite naturally a wide range of competence in it depending upon the nature of their interaction with the native speakers. Though the linguistic repertoire of Dakkhini Urdu speakers would also include Hindi and English, we were interested in exploring the symbolic significance of Dakkhini Urdu, Standard Urdu and Kannada for characterization of identity at different levels. We were interested in finding out, on the one hand, what significance was accorded to Standard Urdu as a symbol of supra-local identity with Muslims in other parts of the country, and to Dakkhini Urdu as a symbol of separate identity at the local level on the other. As regards Kannada,we were interested in finding out whether the extension of the use of Kannada in the family domain will lead to the disruption of the distinctiveness of Dakkhini Urdu speakers as a community.

As a further projection of the symbolic significance of Standard Urdu, it was further considered on two dimensions. First, since Standard Urdu is used in formal domains, we were interested in studying its relevance as a mark of social status. This was correlated to its actual use and to attitudes towards its use in the family domain,which presents a contrast with Dakkhini Urdu. Second, the integrative function of Standard Urdu for identity and social status was set against its instrumental function. In this respect Standard Urdu presents a contrast with the instrumental function of Kannada. It was decided to find out whether the Dakkhini Urdu speakers accord equal or differential importance to these two languages from the point of view of their instrumental function, and whether the perception of their significance fits in well with the attitudes towards them with regard to their use and functions in other social contexts. It was hypothesized that any dissonance we may find in the attitudes of Dakkhini speakers between the instrumental and integrative functions of language will have implications for linguistic minorities in general, and the solution of linguistic problems of Dakkhini Urdu speakers in particular. Keeping the above considerations in mind, the data on the following questions have been analysed in the present study:

1. Would you consider that the use of Standard Urdu by you gives you a sense of unity with the Indian Muslims in other parts of the country?

2. Would you consider that the use of Dakkhini Urdu in your family helps you in maintaining a separate identity from Muslims in other parts of the country?

3. Would you consider that the use of Kannada in your family will disrupt the distinctiveness of your community?

4. Would you consider that the use of Standard Urdu is a mark of social status?

5. Would you consider that a knowledge of Standard Urdu helps you in getting jobs?

6. Would you consider that a knowledge of Kannada should be essential in getting jobs?

The above questions constitute a part of questionnaire B which consists of 26 questions, and which elicits responses on a five-point scale ranging from strong favour to strong opposition, with a neutral point, about attitudes towards Dakkhini Urdu, Standard Urdu and Kannada regarding their use in education, family, literature and cultural activities as well as their symbolic significance for identity, social status and instrumental functions. Similarly questionnaire A, consisting of 19 questions, elicits data regarding the frequency of use of these languages on the five-point scale ranging from maximum to minimum. In the analysis and interpretation of the questions given above, we will refer to findings about other questions, wherever necessary.

The data were collected from 250 respondents, which constitutes 3.5% of the total Muslim population of Mysore city. The variables of sex, age, education and occupation were taken into consideration in drawing the sample design. Three categories of education, ie below high school, below graduation, and graduation and above, and three categories of age, ie below 25 years, between 25 and 40 years, and above 40 years, were considered significant for the present project. Similarly, six occupational categories, students, teachers, businessmen, office-goers, workers including drivers, craftsmen, machine operators etc, and professionals including doctors, lawyers, journalists, scientists and lecturers, were found to be representative of the community for our purpose. The data in terms of age, education and occupational categories in respect of male respondents have been subjected to one-way analysis of variance for each question separately to study group differences. We have also made use of simple frequency percentages in the interpretation of the data.

The analysis of data about the first three questions shows that, while both male and female respondents consider Standard Urdu as a symbol of unequivocal significance for identity with Muslims in other parts of the country, they seem to be divided over the function of Dakkhini Urdu and Kannada for identity purposes. A clear pattern emerges if we merge the percentages of responses at the two positive points of the scale as well as the two negative ones. Thus, 91.26%

male (N-183) and 91.67% female (N-60) respondents regard Standard Urdu as a symbol of supra-local identity. As opposed to this, 57.38% male and 58.34% female respondents think that Dakkhini Urdu helps in maintaining identity at the local level, and 32.78% and 31.67% respectively have an opposite opinion. Similarly, 43.17% male and 30.00% female respondents are of the view that the use of Kannada in the family will destroy their distinctiveness as a community, and 40.98% and 56.57% respectively do not consider it as a threat to their identity. The identity function of Dakkhini Urdu seems to be of doubtful value, though a majority of respondents have expressed their views in its favour. Though Dakkhini Urdu had a literary tradition in the earlier periods of history, at present it is used neither in education nor in literature or mass communication, except for some efforts in this direction to be noted below. Similarly, it is not considered as a mark of social status and prestige. In view of these facts, the salience of Dakkhini Urdu for identity at the local level seems to be very much restricted. However, some creative writers want to revive it as a literary medium, and have used it in poetry and drama. As yet, not only is the use of Dakkhini Urdu not acceptable in formal settings, but it is also considered undesirable from the point of view of acquisition of norms of Standard Urdu.

The attitudes towards Kannada show the kind of predicament in which a linguistic minority may find itself in its relationship with the majority community. On the one hand, the minority community does not want to perceive the majority community as a threat to its survival and identity. If it does, it may generate conflict and lead to rivalry depending upon its strength and social mobilization. Similarly, a linguistic minority cannot ignore the advantages accruing to it by accepting the majority language. On the other hand, a minority cannot accept the majority language and its culture wholesale, as it will lead to its assimilation and ultimately to loss of its identity. The Dakkhini Urdu speakers may not consider the increasing use of Kannada in the family as a threat to their identity, as they are sufficiently distinguished from the majority Kannada community in terms of religion and socio-cultural organization. And yet they do not accept it as a medium of education. They are in favour of Kannada only as a subject in the school curriculum (See Dua, 1980a). This ambivalence in attitudes of the linguistic minority towards the majority language will become clearer in the analysis of the last two questions mentioned above.

Before considering the response to the first three questions in terms of one-way variance analysis, two comments need to be made regarding the five-point scale. First, we do not find any difference between male and female respondents, as is clear from the percentages given above when we merge the two positive or negative points of the scale. However, when the percentages of the positive or negative points of the scale are not merged, a clear distinction may be noted between male and female respondents. This is particularly evident in the case of the first question regarding Standard Urdu as a symbol of supra-local identity. Thus, while the percentages of males at the 'strongly favour' and 'favour' points of the scale are respectively 69.40% and 21.86%, the corresponding percentages for females are 51.67% and 40% respectively. This may be interpreted as

showing that the male respondents may be more aware of the symbolic significance of Standard Urdu than the females. Secondly, it seems to be generally the case that the percentage of response at the 'oppose' point of the scale is higher than at the 'strongly oppose' point. For instance, 24.59% males and 41.67% females 'oppose', and 16.39% and 15.00% respectively 'strongly oppose', the view that the increasing use of Kannada in the family will destroy their identity. On the basis of these two comments it might be said that the five-point scale can be useful in bringing out differences between social groups, as well as the division of opinions and attitudes on social issues.

The one-way analysis of variance for studying the group differences in respect of male respondents according to the categories of age, education and occupation shows that age groups differ significantly in their attitudes towards the symbolic function of Standard Urdu, educational groups differ significantly in their attitudes towards Kannada, and occupational groups in their attitudes to all the three languages.

As regards age-group differences we find that as the age of the respondents goes up, they tend to have a more strongly favourable attitude towards Standard Urdu as a mark of supra-local identity with Muslims in other parts of the country as shown by 51.22% males below 25 years, 69.14% between 25 and 40 years, and 81.97% above 40 years. It is interesting to note that while age groups do not differ significantly in their attitudes towards the use of Standard Urdu in education, family, mass communication and literature, they show significant differences as regards its symbolic function for supra-local identity.

The difference among the educational groups in their attitudes towards the increasing use of Kannada at home shows two facts in terms of simple frequency percentages. First, the respondents with low education, ie below high school, strongly think that the use of Kannada in the family will disrupt the distinctiveness of their community in comparison to highly educated respondents as shown by 65.67% as against 21.18% of the highly educated group. Secondly, 24.24% highly educated respondents express 'no opinion' on the issue, whereas the percentage of 'no opinion' in the case of low educated respondents is zero. Moreover, the percentage of those who do not see Kannada as a threat to their identity is higher among the highly educated respondents (40.81% on merging the two negative points of the scale) than among the respondents with low-level education (21.88% only). The middle educated group has similar attitudes to the highly educated group. The only explanation for this difference between the educational groups seems to be that the group with low-level education is generally more emotionally involved in the issues of language use for identity than the highly educated group. This observation is also supported by the fact that the educational groups differ significantly in their attitudes towards the use of Standard Urdu as a medium of education, in that the highly educated group 'strongly favour' Urdu medium less than the poorly educated group.

Though educational groups do not differ significantly in their attitudes towards Dakkhini Urdu as an identity symbol at the local level, we find the same kind of trend regarding the difference between

the poorly and highly educated groups as noted above in the case of attitudes towards Kannada. Thus, 40.62% of respondents with below high school education 'strongly favour' Dakkhini Urdu as a symbol of identity at the local level, whereas the corresponding percentage for highly educated respondents is 15.15% only. Similarly, the percentage of 'no opinion' among the highly educated respondents is 21.21% as against 3.12% among the poorly educated respondents. Again, we might say that the poorly educated respondents are more emotionally involved in the question of language as an identity token than the highly educated respondents. The latter may be more rationally motivated in their behaviour due to economic and social advantages associated with language issues.

As educational differences are associated with various occupational categories, the above facts must be considered in relation to the attitudes of the occupational groups which differ significantly in the case of all the three languages. Among the occupational groups the professionals stand apart from all the other groups, as regards their attitudes towards the three languages. In the case of Standard Urdu as a mark of supra-local identity, the percentage of professionals who 'strongly favour' Standard Urdu is 56.76% which is lowest in comparison to other groups; 13.51% of them express 'no opinion' which is the highest in comparison to other groups. As regards the attitudes towards Dakkhini Urdu and Kannada, the professionals show a marked contrast with the workers. Thus, the highest percentage of workers, ie 42.11%, 'strongly favour' Dakkhini Urdu as an identity token at the local level, whereas the professionals show the lowest percentage, ie 8.11%. Similarly, the highest percentage of workers, ie 50%, 'strongly favour' the view that the increasing use of Kannada in the family will destroy their distinctiveness as a community, whereas only 21.62% professionals have this view. Another fact to be noticed regarding the attitudes of the professionals concerns the range of non-commitment or negative attitudes. For instance, 21.62% professionals express 'no opinion' as regards their attitudes towards the symbolic function of Dakkhini Urdu or to the threat involved in the increasing use of Kannada. This percentage is the highest in comparison to other groups. Again, the highest percentage of professionals, ie 48.65% (on merging the two negative points of the scale), do not consider the increasing use of Kannada as a threat to their distinctiveness as a community. Similarly, except for students, the highest percentage of professionals, ie 35.07% (on merging the two negative points of the scale), do not consider Dakkhini Urdu as an identity token at the local level. In view of the low-profile, non-committal attitudes of the professionals regarding the symbolic functions of Standard Urdu, Dakkhini Urdu and Kannada, it might be interesting to ask whether their attitudes reflect the social reality in which the minority community finds itself, whether they could be effective opinion makers in the process of mass mobilization, and whether language symbols could be equally adequate and potent for all sections of the community.

The factors that contribute to making a language a mark of social status and prestige may vary from speech community to speech community and may not have equal significance for all sections of people within the same community. In this respect the data on the study of Standard Urdu as a mark of social status show an interesting situation about the Dakkhini Urdu speakers. It is quite obvious that about 80% to 90% of both male and female respondents (on merging the two positive points of the scale) consider Standard Urdu a mark of social status. It seems to be quite understandable, as Standard Urdu is used in education, literature, mass communication and in formal settings. The actual use of Standard Urdu is almost negligible in the family domain. However, all respondents show a highly favourable attitude towards the use of Standard Urdu at home. This situation may have serious consequences for the survival of Dakkhini Urdu. It may be asked whether the increasing use of Standard Urdu and highly favourable attitudes towards its use would lead to the displacement of Dakkhini Urdu by Standard Urdu. Though the function of Dakkhini Urdu as an identity token at the regional level and its restricted use in literature may indicate its survival, the possibility of its displacement under the pressure of Standard Urdu cannot be ignored. However, this question will be answered only in the future.

The one-way analysis of variance regarding the response about the symbolic function of Standard Urdu for social status shows significant differences among age groups and occupational groups. Though educational groups do not differ significantly, some relation between the educational level of the respondents and their attitudes can be established in terms of simple frequency percentages.

In terms of age, we find that as the respondents grow in age they tend to perceive the significance of Standard Urdu for social status to be more, in comparison to younger generation respondents. Thus, while 58.54% male respondents with age below 25 years 'strongly favour' the view of Standard Urdu as a marker of social status, the corresponding percentage for the respondents with age above 40 years is 81.97%. A similar trend was noted above in connection with age-group differences, regarding the function of Standard Urdu as a symbol of supra-local identity. It is not certain whether this indicates a generation gap or whether the awareness of the symbolic functions of language for identity and social status develops at a relatively higher age in life. The explanation of the generation gap may not be relevant in the present context, as the age groups do not show any significant differences regarding their attitudes towards the use of Standard Urdu in other contexts. How far the awareness of the symbolic functions develops in later age needs to be explored in other settings before it is accepted as a general fact.

The differences between occupational groups regarding the function of Standard Urdu for social status again show that the professionals stand apart from other groups. All occupational groups except students and professionals 'strongly favour' Standard Urdu as a mark of social status. In the case of students, it might be said that they may not have acquired the awareness of Standard Urdu as being a marker of prestige and social status. But this

cannot be so in the case of professionals. We might offer a general explanation here. In the social organization, different social groups have different reference points and different social markers for evaluating their social status with respect to each other. As the professionals do not value Standard Urdu as a medium of education, as well as an identity symbol, as highly as other groups, they may accord less importance to it as a marker of social status as well.

Though educational groups do not differ significantly, they show the same trend as noted above in connection with their attitudes towards Kannada or Dakkhini Urdus in that lower percentage, of highly educated respondents 'strongly favour' Standard Urdu as a mark of social status in comparison to below high school respondents.

The attitudes of the Dakkhini Urdu speakers towards the instrumental function of Standard Urdu and Kannada present an entirely different picture from what we find in the analysis of the first four questions given above. We find a paradoxical situation which may be characteristic of all linguistic minorities in India and elsewhere. It is quite obvious that Dakkhini Urdu speakers consider Standard Urdu a symbol of supra-local identity and social status, and are favourable to its use in education as a medium of instruction and in literature and cultural activities. Thus, Standard Urdu has a great significance for them as far as its integrative function is concerned. But they do not give much importance to it for its instrumental function, as very few job opportunities are available to those who are educated in the Urdu medium or who graduate in Urdu language and literature. For instance, only 22.40% male respondents 'strongly' think that knowledge of Urdu helps in getting jobs, 15.85% have 'no opinion' in the matter, and 20.22% 'oppose' that it helps in getting jobs. As against this, 48.09% male respondents 'strongly favour' that knowledge of Kannada should be made essential in getting jobs, 4.92% have 'no opinion' in the matter and 7.10% 'oppose' the view. Two observations can be made here. It might be said that the attitudes of Dakkhini Urdu speakers reflect the fact that a knowledge of Kannada has been declared essential for getting any job in government departments. Alternatively, it might be said that they realize the utility of the majority language and therefore not only want it to be taught as a subject, but also consider it essential for getting jobs. This may have serious consequences for the community. The low instrumental value of Standard Urdu may not only erode its integrative function in the long run, but also create a sense of frustation and alienation among the people educated in Urdu language and literature. This may give rise to tendencies of revolt or lead to assimilation. It is in this context that it is said that linguistic minorities need to be given not only equality of treatment but also 'a measure of differential treatment in order to preserve the basic ethnic and linguistic characteristics which they possess and which distinguish them from the majority of the population' (Verdoodt, 1975).

The one way analysis of variance does not show any significant group differences among respondents in terms of age, education, and occupation regarding their attitudes towards the instrumental function of Standard Urdu and Kannada. However, a few observations need to be made on the basis of percentage distribution among the educational and occupational groups. Among the educational groups the percentage

of respondents who consider that a knowledge of Standard Urdu helps in getting jobs goes down as their educational qualification increases. Thus, 81.25% (on merging the two positive points of the scale) of respondents with less than high school education think a knowledge of Standard Urdu useful in job opportunities, whereas the corresponding percentage for the respondents with BA and above is 43.94%. This seems to be quite expected since it would be the people educated in Urdu who might have the experience of not getting any job opportunities. Similarly, among the occupational groups, the lowest percentage of professionals in comparison to other groups, ie 37.84% (on merging the two positive points of the scale) consider a knowledge of Standard Urdu helpful in getting jobs. This again seems to be quite expected in view of their special qualifications which would not generally include a knowledge of Standard Urdu. Similarly, in comparison to other groups, the lowest percentage of both the highly educated group and the professionals favour the view that a knowledge of Kannada should be made essential in getting jobs. This may be because they consider this requirement discriminatory in view of their educational and professional qualifications essentially required for the jobs in question. If that is the case, a measure of differential treatment seems to be a relevant principle for preserving the interests of linguistic minorities.

To sum up, the following points emerge as significant from the analysis of data presented above regarding the attitudes of Dakkhini Urdu speakers towards the symbolic functions of standard Urdu, Dakkhini Urdu and Kannada:

1. Standard Urdu is unequivocally regarded as a symbol of supra-local identity with Muslims in other parts of the country.

2. The function of Dakkhini Urdu as an identity token at the local level is not very sharply defined, and may be taken over in future by the extended use of Standard Urdu.

3. The increasing use of Kannada is not considered as threatening the distinctiveness of the community as a whole, though it cannot be ruled out that the perception of the high instrumental value of Kannada on the part of the minority community and pressure on the part of the majority, may have serious consequences for the development of Standard Urdu and socio-cultural integration of the Dakkhini Urdu speakers.

4. The significance of Standard Urdu for instrumental function is perceived to be low in comparison with its role for identity and social status, as well as with the instrumental function of Kannada. This situation requires not only equality of treatment but also a measure of differential treatment in the solution of language problems faced by linguistic minorities.

5. From the point of view of one-way analysis, the category of occupation seems to be most significant as it brings out significant group differences as regards attitudes towards the integrative functions of Standard Urdu, Dakkhini Urdu and

Kannada. Age groups differ significantly in their attitudes to Standard Urdu as a symbol of supra-local identity and social status, and educational groups in their attitudes towards the disruptive effect of Kannada only.

6. Among the occupational groups, the professionals are distinguished from other groups in that they accord less importance to Standard Urdu as a symbol of supra-local identity and social status and for its instrumental function, and to Dakkhini Urdu as an identity token at the local level. The role of the elites for the social mobilization of Dakkhini Urdu speakers seems to be diffuse and uncertain.

7. The less educated common people seem to be more emotionally involved in language issues pertaining to identity, social status and employment than the highly educated professionals.

8. The symbolic functions of language may not have equal significance and relevance for all sections of a speech community under certain social conditions.

FOOTNOTE

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REFERENCES

Brass, P R
1975 Language, Religion and Politics in India, Delhi: Vikas Publishing House.

Dua, H R
1980a 'Language attitude and language identity: A case study of Dakkhini Urdu speakers in Mysore', in To Greater Heights, Mysore: CIIL.

1980b 'Language use, language attitudes and language identity among linguistic minorities: A case study of Dakkhini Urdu Speakers in Mysore'. Project Report, CIIL.

Fishman, J A
1977 'Language and ethnicity', in H Giles (ed.) Language Ethnicity and Inter-group Relations, London: Academic Press.

Kelman, H C
1971 'Language as an aid and barrier to involvement in the national system', in J Rubin and B Jernud (ed.) Can Language be Planned? Honolulu: Hawaii U.P.

Sutherland, M B
1979 'Comparative perspectives on the education of cultural minorities', in A E Alcock, B K Taylor and J M Welton (ed.) The Future of Cultural Minorities, London: Macmillan.

Verdoodt, A
1975 'Ethnic minorities and the United Nations', in W Mackey and A Verdoodt (ed.) The Multinational Society, Rowley, Mass: Newbury House.

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NEW WORLDS TO CONQUER IN THE SOCIOLOGY OF LANGUAGE

As we toast Bob Le Page's sixtieth birthday, and give thanks for his continuing good health and continuing fine contributions to the language sciences, we can also make use of the opportunity that his birthday presents in order to rethink and re-evaluate the score or more years that have gone into the modern sociolinguistic enterprise. As with all new fields, three basic questions can be asked concerning them: Why did they arise? Why did they develop in the particular directions that they have evinced? Have they stabilized or are they ephemeral and destined to be subsumed by other disciplines? Ever since 1959, when I taught my first course in the sociology of language (it was the second semester of a year-long sequence that I initiated at the University of Pennsylvania and that came to be entitled 'Human Relations and Language' since the Linguistics Department's representative refused to permit it to be called 'Language and Human Relations'), I have been watching this field rather closely. I had earlier searched for this field, as a doctoral student, and had been advised to go into social psychology instead, because (so my advisers believed) there was 'no such field as the sociology of language'. Now, as we approach the end of the 70's, it is not only evident that such a field exists but that it is robust enough to face some rather searching questions as to its identity and its future as well.

Why?

Kjolseth (1977) has contributed provocatively to the question of why the sociology of language arose in the late 50's and early 60's. It was a time when several societal concerns received a new intellectual legitimacy. Populations that had hitherto been neglected came into far greater prominence in academe and on the intellectual horizons of academics. Not only did poverty, racism, ethnic distinctions and (mis)education become distinctly more pressing areas of governmental and university attention, but those who bore within themselves the markings of the foregoing disadvantages became ever so much more noticeable within the academy itself - both as faculty and as students - as the wave of college-going crested. The GI Bill, the Civil Rights movement, the growth of public higher education, the flood of post-war babies onto the campuses, all contributed to a growing realization that many of the problems that plagued society and that puzzled academicians had strong language components, and that the language sciences would do well to attend to them. There was a need for the sociology of language. If it did not exist, it was the proper time for it to be created.

However, social and intellectual need are not enough and were not the only forces bringing this new area of specialization into being. The inability or unwillingness of the more traditional

language-pertinent disciplines to tackle the above language-related topics was also quite apparent (Shuy and Fasold, 1971). The sociology of the day virtually ignored language and, therefore, was aware of it neither as a symbol, a process or a problem. The linguistics of the period trivialized society (and, therefore, language as well), while going through its own upheaval in the direction of even further detachment from societal reality. The anthropology of the time was still focused on one or another aspect of ethnolinguistics, whether on Whorfianism *per se* or on folk taxonomies as the most demonstrable but weakest level of the language and culture link. Thus, at the very time that new perspectives were most needed on language in society, the traditional disciplines were singularly inhospitable to such perspectives. Since intellect abhors a vacuum every bit as much as does the rest of nature, the sociology of language was fostered not only by need but by the unwillingness of the traditional disciplines to meet this need.

Finally, it should be noted that other language-related areas of specialization were also arising and flourishing at the very same time: psycholinguistics, neurolinguistics, quantitative linguistics, machine translation, applied linguistics more generally. Truly, it was a glorious time in many ways - a time of seemingly endless support for research, training, conferences and publications. All of this made it possible for the sociology of language to be more quickly recognized and to develop more rapidly its unique stress on variation in language and social behaviour, to pioneer several conceptual and methodological innovations, and to achieve its own social organizations at the national and international level, journals, book-series, and full-fledged degree programmes.

Directions

I have previously commented on the directions in which our young area of specialization has developed during the sixties and seventies (Fishman, 1972). My view is that it was (and is) overly dominated by three different kinds of disciplinary extremes. The revolutionaries who created the new field nevertheless bore the marks of their own individual prior disciplinary socializations and, as such, they were a peculiar breed of revolutionaries: namely, revolutionaries with establishment tendencies of 'reversion to kind'.

Extreme A pursues the self-annulling hypothesis that sociolinguistics is no more than a broader, better variety of general linguistics. It will carry linguistics 'on beyond zebra', or beyond the sentence, to paragraphs, to conversations, to communication *per se*. Presumably, just as the state itself is deemed to wither away, so code-anchored linguistics too will dissolve and the sociolinguistic enterprise will arise in its stead as the true linguistics, simultaneously the aristocracy, the proletariat and the saviour of the language sciences, bringing to the fore the gospel of new ways of analysing variation in ever longer and more complex corpuses of natural speech. Thus, the duty and the fate of sociolinguistics is to sacrifice itself on the altar of linguistics as a whole.

Extreme B carries within itself the self-eclipsing limitation to microprocess analysis at all costs and in all cases. It basically

seizes upon the sociolinguistic enterprise as an avenue for developing a sociology of everyday understandings. From ethnography to ethnomethodology its precision is so great and its attention to detail so complete that it is understandably grumpy when confronted by the observation that it has not only overlooked polities (the spatial and political dimension) and centuries (the time dimension) but also social classes, the difference between social institutions and the compositing of normative roles and occasions. As every interaction is minutely and painfully different from any that occurred before (or after), anecdotal and analytic detail asymptotically approach (but never quite reach) some ultimate reality without ever encountering theoretical parsimony along the way.

Extreme C involves the self-stultifying fixation upon macrostructure analysis. Admirable sociohistorical studies roam up and down the centuries and the continents without once encountering or revealing any contextualized 'who said what to whom, when and to what end' at all. There are repertoires galore, but no metaphor. Demography triumphs over variations at every turn. Language has finally been admitted into the paths of society but there is still no speech. Societal allocation of functions reigns supreme.

There may be other extremes as well, eg a social psychological extreme that deals well nigh exclusively with attitudes, values, images, dissonances, etc. However, my point remains the same: in its youth, the sociolinguistic enterprise has been splintered by revolutionary leaders departing from their own pre-sociolinguistic establishments all too briefly and then returning to them with all too little of the total sociolinguistic treasure. Since each extreme represents a pre-sociolinguistic skewing, none of them attains the interdisciplinary potential of the field. The field thus moves only slowly, fitfully, away from the limitations of its disciplinary predecessors, even though they were precisely the limitations that were co-responsible for the appearance of the field as such.

This must be a very generalizable problem in the sociology of knowledge. Can monodisciplinary fathers build a new, multidisciplinary field? Can the 'linguistics first', 'microprocess' and 'macrostructure' biases be bridged more frequently? Can a new society be built by those who were socialized in the old? Can the new ship be built out of the timbers of the old ship at a time when both are on the high seas? Obviously, it is not easy. The building of interdisciplines requires conviction, support and time and the sociolinguistic interdiscipline has had all too little of any of these. Perhaps it has succeeded 'too far and too fast', without developing a truly strong interdisciplinary nature. At any rate, this still remains a promissory note that has yet to be seriously addressed.

Stabilization

During the past quarter century many fledgling areas within the language sciences have atrophied and disappeared. Exolinguistics has vanished and no one sheds a tear for it. Ethnolinguistics and even anthropological linguistics seem no longer to generate the positive commitment that they once enjoyed and have about them

that somewhat archaic ring that presages further decline. Can this happen to the sociology of language as well? Various sources of enfeeblement must be recognized and grappled with if this is not to occur.

The intellectual enfeeblement of disciplinary encapsulations has already been referred to. Perhaps it need merely be stressed that *this* danger is disproportionately great from the direction of linguistics. Indeed, if there were more of a 'sociological threat' the danger from linguistics would be correspondingly reduced. However, both of the sociological establishments alluded to above are far less concerned with language than is linguistics. Our failure to impress sociology is, indeed, one of the greatest failures of the sociolinguistic enterprise. This failure is understandable in terms of the amorphous, unsystematized nature of the sociological beast, such that it is hard to hit it at its vital centre given that it is so hard to tell where or what that might be. At any rate, language as a clue to and as an aspect of group formation, group boundaries, role boundaries and task boundaries is still studied as little (or even less than) language as an aspect of societal behaviour. Unless and until there is a serious sociological investment in all sociolinguistic pursuits, the field as such must inexorably gravitate toward linguistics and become a colony or a satellite thereof.

Another obvious source of destabilization in the sociology of language is the fact that budgetary cutbacks have hit it at such an early point in its development that it is in danger of becoming a 'one course field'. Clearly, it enjoys too few centres of academic depth (not to mention interdisciplinary depth). Once one steps outside of the full-blown programmes at the University of Pennsylvania and Georgetown University, one enters the sympathetic (but not always programme-formalized) environs of the University of Texas, Berkeley and Stanford. Once one leaves those hallowed grounds as well, even sympathy thins out appreciably and the one course syndrome begins and stretches in all directions. There are just not enough points of critical mass with respect to faculty, students, courses, seminars and research for real specialization in this area to be achieved by a new generation. Certainly, the integrated Language and Behaviour model, involving equal attention to linguistics, sociology and sociolinguistics *per se* which marked the now defunct (fiscally assassinated) programmes at Yeshiva University and Hebrew University (Fishman, 1977), has gone the way of all flesh. Its demise strengthened my conviction that interdisciplinary programmes, novel and productive though they may be, must fall upon evil days unless they are co-sponsored, co-budgeted and co-utilized by their disciplinary progenitors, no matter how conflictually Oedipal the relationship between such parents and such offspring may be. Unless such integrated and in-depth programmes are reestablished, the intellectual ferment which has marked this field for two decades must subside and, finally, grind to a halt.

Yet another area of thin ice in the sociology of language is its unexpected aloofness from applied problems. Although such problems were instrumental in bringing the field into being the sociolinguistic investment and presence in bilingual education, language

planning, mother tongue teaching and second language teaching is far more negligible than one would have hoped for. Instead of realizing the sublime validity of the inverted Lewinian dictum 'nothing is as provocative for theory as variation in effective practice', the sociolinguistic enterprise began to view practical problems as no more than that: an endless array of separate problems. Perhaps this tendency is no more than the natural consequence of the various self-eclipsing and self-annulling views mentioned earlier. At any rate, without a voluntary *rapprochement* with applied emphasis (such as those that linguistics, anthropology and sociology all find so useful) the sociology of language will not only surrender its birthright but its growth potential.

Finally, I must mention the sin of Americanism in particular and of Westernism in general. The sociology of language suffers from Americanism masquerading as universalism. Here I not only refer to Bob Le Page, whose work lacks the recognition in the USA that it has achieved elsewhere, but to the overlooked work of countless other European and, even more, non-European workers in the sociolinguistic vineyards. As a result, sociolinguistic research and theory reflects yet another Western bias, albeit often an unconscious one, with respect to such variegated phenomena as ethnicity, religion, poverty, and other similarly fundamental language-encumbered societal structures and processes. Most of the problems of the field derive from the problems of its American component since it is that component that is clearly numerically predominant. A more conscious infusion of non-American and non-Western effort is absolutely crucial if yet another form of ethnocentrism and colonialism is not to be perpetrated on the pursuit of knowledge and understanding.

The Bottom Line

I have concentrated so much on criticism that I must conclude by stressing that I am by no means disillusioned with the sociology of language. If it has not been an unmitigated success it has nevertheless proved itself to be far more than a flash-in-the-pan. If its future is still uncertain, it is an uncertainty with respect to its fully realized, multi-splendoured potential rather than an uncertainty of existence *per se*. I fully believe my last twenty-some years have not only been well-spent but that they have been thrillingly spent helping the sociology of language conquer whole new worlds of intellectual challenge. It is no disrespect to the field (quite the contrary, it is a reflection of my great faith in it) if I conclude that it still has further new worlds to conquer. These new worlds are primarily within its interdisciplinary self. Bob Le Page will doubtless be among the small, intrepid band that will lead us all in the direction of the difficult frontiers ahead, toward greater interdisciplinarity, greater depth, greater application and greater non-Western exposure. These are all the hallmarks of his own work and, hopefully, they will become more representative of the field as a whole during the twenty-some years immediately ahead.

REFERENCES

Fishman, J A
1972 'Problems and prospects of the sociology of language', in A S Dil (ed.) Language in Socio-cultural Change, Stanford: U.P., 268-85.

1977 'A graduate program in the sociology of language', in J A Fishman (ed.) Advances in the Creation and Revision of Writing Systems, The Hague: Mouton, 471-74. Also in J A Fishman (ed.) Advances in the Study of Societal Multilingualism, The Hague: Mouton, 795-98.

Kjolseth, R
1977 'The development of the sociology of language and its social implications', in J A Fishman (ed.) Advances in the Study of Societal Multilingualism, The Hague: Mouton, 799-825.

Shuy, R and R Fasold
1971 'Contemporary emphases in sociolinguistics', in R J O'Brien SJ (ed.) Linguistics: Developments of the Sixties -- Viewpoints for the Seventies, Washington DC: Georgetown U.P., 185-97.

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ACCOMMODATION THEORY: SOME NEW DIRECTIONS[1]

Sociolinguistics has made great strides in informing us when, how and on which linguistic levels we modify our speech (Ervin-Tripp, 1969; Hymes, 1972). Little attention, however, has been paid to *why* we do this. The explanatory power of sociolinguistics in this respect has mainly been in terms of norms and rules. It is true that there are many explicit, and sometimes strong implicit, social norms which dictate how we should speak appropriately, particularly in bi- and multilingual settings (Herman, 1961). However, when this normative analysis appears to be the only one invoked, it gives the impression of a speaker as a kind of 'sociolinguistic automaton' (Giles, 1977a). What of people's moods, motives, feelings and loyalties, do not these more dynamic elements also shape our linguistic behaviour? It has been argued elsewhere that by highlighting such factors and by making use of current social psychological knowledge, sociolinguistics may be given a new lease of life and directed towards a richer theoretical base (Giles and St Clair, in press). Therefore, by drawing upon social psychological theory, a conceptual framework has been derived called 'accommodation theory' which may be flexible enough ultimately to cope with speech diversity in a wide variety of verbal and sociocultural contexts (Giles, 1973a, 1977a, Giles and Powesland, 1975). It is a great pleasure to discuss these notions within the context of the present volume given that insightful aspects of Robert Le Page's work on language behaviour in multilingual communities may be regarded as an important forerunner of accommodation theory (Le Page, 1968).

A basic postulate of accommodation theory is that people are motivated to adjust their speech styles, or accommodate, as a means of expressing values, attitudes and intentions towards others. It is proposed that the extent to which individuals shift their speech styles toward or away from the speech styles of their interlocutors is a mechanism by which social approval or disapproval is communicated. A shift in speech style toward that of another is termed convergence and is considered often a reflection of social integration, whereas a shift away from the other's style of speech represents divergence and is considered often a tactic of social dissociation. Our aim in this chapter is not only to elaborate theoretically on the complexities of speech convergence and divergence published so far, but also to introduce a further concept into accommodation theory, that of speech *complementarity*.

CONVERGENCE

When two people meet each other for the first time there is a tendency for them to become more alike in their languages (Giles, Taylor and Bourhis, 1973), pronunciations (Giles, 1973a), speech rates (Webb, 1970), pause and utterance lengths (Jaffe and Feldstein, 1972),

vocal intensities (Natalé, 1975a), the intimacy of self-disclosures (McAllister and Kiesler, 1975), non-verbal behaviours (von Raffler Engel, 1980) and so forth. In short, to converge (cf notions of response matching, (Argyle, 1969) and congruence (Feldstein, 1972)). Indeed, many interpersonal speech shifts traditionally viewed as rule-governed, for example, adult-to-child, young-to-old, male-to-female, and speech to foreigners can be subsumed under an accommodation rubric. Moreover, the vast literature on language and dialect assimilation among immigrant groups into an alien dominant culture can also be viewed in such a fashion (Fishman, 1966; Giles and Bourhis, 1976). In all these cases, people may be converging their speech to how they believe others in the situation would best receive it. It is suggested that this phenomenon may profitably be considered as a reflection (often non-conscious) of a speaker's or a group's need for social integration or identification with another (Le Page, 1968).[2] In similar vein, Lambert and Gardner (1972) and Gardner (1979) have pointed to the importance of an integrative motive in the effective acquisition of another group's language (cf Taylor, Meynard and Rheault, 1977).

Let us examine convergence more closely in the light of three social psychological processes: similarity-support-attraction; social exchange; and causal attribution (cf Giles, 1977a).

Similarity-support-attraction processes

In its simplest form, similarity attraction theory proposes that the more similar our attitudes and beliefs are to certain others, the more likely it is that we will be attracted to them (Byrne, 1969; Rokeach, Smith and Evans, 1960). Speech convergence is but one of the many devices a person may adopt in order to become more similar to, and hence likeable for, another. Specifically, it involves the reduction of linguistic dissimilarities between two people in terms of their languages, syntactic and paralinguistic features, etc. Since increasing similarity between people with regard to such an important parameter as communication is likely to increase attraction as well as intelligibility (Triandis, 1960), speech convergence perhaps reflects a speaker's desire for social approval.

A theoretical extension of this position comes from the work of Berger and Calabrese (1975). They have argued that when strangers meet, their primary concern is one of reducing uncertainty about each other. It is suggested that one of the goals of initial interactions is to predict the likely behaviours of the others in the situation so that appropriate modes of behaviour can be selected from the person's repertoire. Less uncertainty in such a situation would lead to a more fluid interaction and increased liking for one's interlocutor. Since increasing similarity leads to greater perceived predictability, speech convergence then should reduce uncertainty and facilitate attraction further.

It would perhaps be useful to raise a question as to what, more specifically, interactants are trying to reduce uncertainty about? A factor analytic study by Berger, Weber, Munley and Dixon (1977) suggests that people may be looking for supportiveness in the behaviour of another when deciding how attractive they might be for them in formal as well as in informal role relationships (cf Duck, 1977).

The types of support found to be important in this respect include perceived understanding, loyalty, concern for the other's welfare and help in reaching goals. It can be argued that the perception of convergence from another not only increases similarities, intelligibility and predictability, but also perceived supportiveness in the sense that this speech shift may be viewed as an important communication concession. Thus, speech convergence, as summarized schematically in Figure 1, can lead to increased attractiveness in the eyes of the recipient by means of a number of overlapping, intervening processes, and arises when such positive social approval is desired from the recipient.

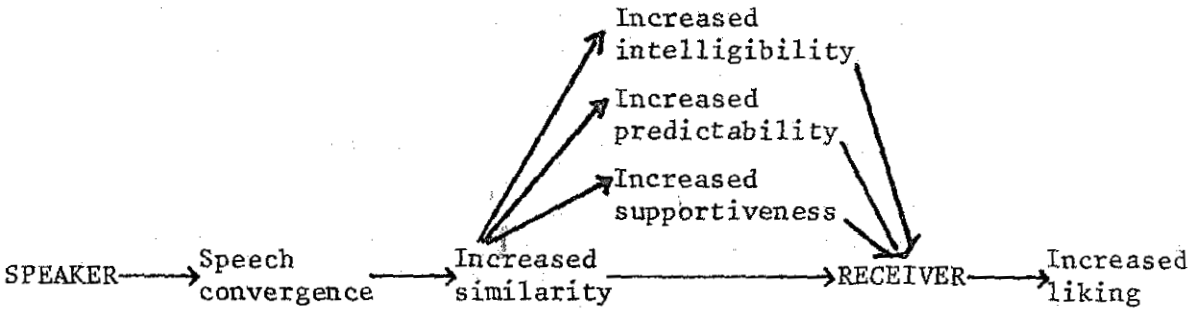

Figure 1. The similarity support attractiveness process

If one accepts the notion that people find approval from others satisfying it would not seem unreasonable to suppose that there may be a general tendency for people to converge their speech towards others in many situations. It follows then that the greater the speaker's need to be attractive to another and hence to display perceived support, the greater the magnitude of convergence there will be. Many factors could affect the intensity of such a need, including the probability of future interactions with the listener, the extent of his or her social power, recollections of previous convergences from that person, etc. Moreover, Natalé (1975a, 1975b) has found that speakers with high needs for social approval converge more to another's vocal intensity and pause length than those with low needs for social approval. Furthermore, it has been shown that the greater one's desire for others' approval, the more similar their voices will sound to one's own (Larsen, Martin and Giles, 1977). This cognition of a reduced linguistic barrier between oneself and another, termed 'perceptual convergence', no doubt facilitates the convergence process since the latter will appear a more attainable target towards which to converge (cf Summerfield, 1975). In a similar vein, Welkowitz and Feldstein (1969, 1970) have shown that the convergence of silence durations by dialogue participants appears to be related to the extent to which they mutually perceive their personalities to be similar.

Naturally enough, convergence will not occur if the speaker does not have the necessary repertoire to do this realistically. However, it is likely that speakers will have sufficient flexibility to converge towards another on at least one linguistic dimension or the other. What though would accommodation theory predict specifically with regard to *pronunciation* features when a Scotsman meets a

Welshman for the first time? It is hypothesized that both will modify their accents in their respective ethnic directions. In this way, speech shifts to their basilects will occur strengthening their common Celtic roots and differentiating themselves from English ancestry. If, on the other hand, their dialects were from the outset somewhat mutually unintelligible, convergence to the acrolect would occur where both would modify their speech in a standard (RP) direction. Convergence towards a given linguistic feature of the other will not occur either if that feature is considered socially undesirable or unattractive by the interactant. Examples of such undesirable features may be found in many so-called speech handicaps (eg, stutters and lisps), the use of obscene words or phrases, and the pronunciation patterns of deviantly-perceived social groups. However, it is suggested that when approval of another who possesses speech characteristics deemed unattractive is sought, these will be perceived as less potent, and convergence will occur along other linguistic dimensions not perceived in such a negatively-emotive fashion. Indeed, Larsen, Martin and Giles (1977) conducted a study where listeners were asked to evaluate a poem read by a speaker with an irritating lisp. It was found that the more they desired the speaker's approval the less was the reported magnitude of the speaker's lisp.

Social exchange processes

The similarity-support-attraction model tends to emphasize only the rewards attending a convergent act, that is, an increase in attraction and/or approval. However, it is likely that certain *costs* would be involved too, such as the increased effort made to converge, and a loss of perceived integrity and personal (and sometimes group) identity. Social exchange theory, again in its simplest form, states that prior to acting, we attempt to assess the rewards and costs of alternative modes of action (Homans, 1961). Therefore, if we have the choice of doing, or saying, A or B, we will tend to choose the alternative which maximizes the chances of a positive outcome and minimizes the chances of an unpleasant one. Engaging in convergent speech acts should then incur more potential rewards for the speaker than costs. For instance, the rewards involved in a Black patient converging towards the speech style of a White physician in the *private* confines of the latter's surgery, might well outweigh the transitory loss of ethnic identity (cf Shuy, 1977), whereas the social approval gleaned by a Black adolescent in converging towards a White teacher in the *public* context of a classroom might not, in this instance, repay the consequent costs of ridicule from his peers.

The notion of rewards attending the use of a certain speech style is problematic, but attempts have been made to specify what they might constitute in empirical terms. Moreover, it can be suggested that the specific rewards that may accrue from convergence may depend on the particular linguistic features involved (cf Taylor and Altman, 1975; Miller and Steinberg, 1975). Let us consider pronunciation, and imagine the context of a job interview in which a male applicant has less prestigious pronunciation patterns than his interviewer. One would predict that the prospective employee would shift his pronunciations more in the direction of the interviewer than vice versa, because of their relative needs for each

other's approval. Studies in many cultures have shown that the more prestigious accent one possesses the more favourably one will be perceived on certain evaluative dimensions (Giles and Powesland, 1975). This is particularly true in England where RP-accented speakers are viewed as far more intelligent, self-confident, industrious and determined than regional-accented speakers, even by the latter themselves (Giles, 1971a). In addition, what one has to say will often be considered more persuasive and of a better quality, and also more likely to gain the cooperation of others than had it been voiced in a less standardized accent (Giles, 1973b; Powesland and Giles, 1975; Giles, Baker and Fielding, 1975; Bourhis and Giles, 1976). The pervasive social influence can also be gauged from the following illustration. In October 1976, Jennifer Giles and I undertook an exploratory examination of the speech used in television advertisements on our local commercial station in the south-west of England (cf Courtney and Whipple, 1974; McArthur and Resko, 1975). Out of the hundred different commercials randomly selected at peak viewing time in the evenings, we found that 83% had principal characters and/or the 'voice talkovers' as male RP-accented voices, and a further 6% female RP-accented. In fact, non-RP accents were only heard at all in 15 commercials, and of these five were foreign. Hence in England, the rewards for our applicant converging to the interviewer (a shift termed *upward* convergence) would not only include being more comprehensible to, and liked by the interviewer, but that what he appeared to be, and what he actually said would be more favourably looked and acted upon. Indeed, there is empirical evidence in a number of cultural contexts which supports the notion that people react favourably to those who converge towards them in terms of language, dialect or pronunciation (Feldman, 1968; Harris and Baudin, 1973; Giles, Taylor and Bourhis, 1973; Simard, Taylor and Giles, 1976; Giles and Smith, 1979).

The rewards just suggested for accent convergence in England have, however, been based on male data (cf Kramer, 1978). A recent study investigating people's reactions to English female accented speech suggests that there may be additional rewards for women (Elyan, Smith, Giles and Bourhis, 1978). It was found that not only are RP-accented women rated higher on competence traits than their regional-accented counterparts as are men but that, in addition, they are also perceived as less weak, more independent, adventurous and feminine. In other words, upward convergence for women may glean a greater array of evaluative rewards than the same speech strategy adopted by a male. These findings shed some light on the reasons why women in the United States and Britain adopt more prestigious-sounding speech than men (Labov, 1966; Trudgill, 1974).

One could also imagine situations, such as an industrial dispute in a small family-owned firm, where there might be a greater need on the part of the employer to win his workers' social approval than vice versa. In this case, the employer might be more prone (within the realistic limits of his repertoire) to shift his accent in the direction of his workers than they would to him (that is, to

converge downwardly). More generally, a shift of this nature is undertaken to reduce embarrassment between people of differing status and to prepare a common basis for the communication of ideas and feelings. Naturally, mutual convergence can occur where upward convergence from one person is complemented by downward convergence from the other - if they both desire social integration.

In the above examples of upward and downward convergence, the target persons accommodated to were always consistent in characteristics of social status and speech style. That is, the high status person (employer) possessed an RP accent while the lower status persons (workers) possessed lower prestige speech patterns (Powesland and Giles, 1975; Brandac, Courtright, Schmidt and Davies, 1976). What would happen, however, when inconsistencies arise between social and linguistic attributes as in a case where one would have to address a court judge who spoke with a distinct regional accent? What speech strategy, for example, would our applicant have employed in the job interview had the employer spoken with a less prestigious accent than he? Given the apparent norm that speakers standardize their speech in formal contexts (Labov, 1966; Trudgill, 1974), and the opposing tendency in this instance to converge in order to gain social approval, our applicant might have found himself in a strategic dilemma. Indeed, research suggests that such a discrepant stimulus person causes cognitive discomfort in others (Aboud, Clément and Taylor, 1974). It is predicted that accommodative pressures in such a situation would outweigh normative ones, and that a linguistically diluted form of convergence would emerge.

The rationale here is two-fold. First, it is our assumption that perceptions of contextual formality-informality are to a large extent based on interpersonal (and intergroup) dimensions anyway (Giles, 1973a; Wish and Kaplan, 1977). Although we might justifiably have preconceptions about how formal a context might be prior to being part of it, and what behaviours might be expected of us (Price and Bouffard, 1974), our definition of the situation ultimately depends much on the other interactant's behaviour (cf Schachter and Singer, 1962). Moreover, the more social power the interactant wields, the more likely it is that cues emanating from him will be used to define what is appropriate behaviour in the situation. Second, it appears that high status persons are considered more responsible for, and thought to have more control over, their behaviour (verbal presumably as well as physical) than low status others (Thibaut and Riecken, 1955; Jones and Aronson, 1973). Hence, it is likely that the possession of a low status accent is seen by others as a valued conscious choice on the part of its user to maintain a group identity or to achieve self-distinctiveness. This attribution is particularly likely, given that the stimulus person is not behaving in accord with social expectations (Walster, Aronson and Abrahams, 1966; Koeske and Crano, 1968); that is, a low prestige accent is not stereotypically associated with a person of high social standing. To summarize the argument, the mere use of a non-prestige speech style by a higher status other is likely to reduce the perceived formality of the situation, and also to afford the speaker with cues as to his preferred modes of identification. An intense desire for the other's social

approval in this instance suggests that our applicant would override contextual predispositions to standardize his speech, and instead would converge.

Another inconsistency between social status and speech style could be envisaged where an individual interacts in an informal setting with a lower status other who possesses a language variety having a higher prestige than his own (cf Aboud, Clément and Taylor, 1974; Giles and Bourhis, 1976). An example here would be a university lecturer from a humble background having a drink with his upper class students. In such a situation, a speaker might again face a dilemma; informal contexts suggest less standardized speech, yet accommodative pressures for approval would suggest a more standardized variety. It is hypothesized, as previously, that the latter pressures would outweigh the former, and a linguistically diluted form of convergence would emerge. The rationale is similar to the above case. The mere use of a prestige form of the language by a lower status other is likely to increase the perceived formality of the interpersonal situation, and also offers cues to the speaker as to his preferred modes of identification. A strong need for social approval in this instance would suggest that the speaker would override contextual predispositions towards casual speech patterns, and convergence in an acrolect direction would ensue. The magnitude of this convergence might even be substantial if the speaker himself is upwardly socially mobile in intent and finds the accent prestige discrepancy to be distasteful.

Obviously, empirical work needs to be undertaken to determine the validity of these propositions in both inconsistency paradigms and to investigate the linguistic features of consequent strategies employed. It is true that these inconsistent situations may be encountered infrequently, nevertheless a robust theory should attempt to cope with complex irregularities. In any case, the above analysis that a traditional, positive sociolinguistic relationship between the formality of context and pronunciation prestige is perhaps psychologically naive.

Causal attribution processes

Thus far, it has been argued that speech convergence from speakers is favourably perceived by its recipients. Implicit in our discussion, however, has been the notion that non-convergence from others is unfavourably perceived (Giles, Taylor and Bourhis, 1973). The rationale for this is that people may be inwardly aware of the positive function of convergence and so to decode a lack of it from a speaker means that he does not value the listener's approval. Such a state of affairs is, of course, deleterious to the listener's self-esteem and hence the speaker may feel derogated because of it. However, work to be briefly described below in a bilingual context, where the encoding and decoding of convergence and non-convergence is perhaps more under conscious control than situations hitherto discussed, suggests that the above perceptions of convergence and non-convergence are far too simplistic to operate under all conditions. An elaboration of accommodation theory by means of appealing to processes of causal attribution is felt necessary.

Research on causal attribution theory (Heider, 1958; Jones and Davis, 1965; Kelley, 1973) suggests that we interpret other people's behaviour and evaluate the persons themselves in terms of the motives and intentions that we attribute as the cause of their behaviour. For example, we do not just observe a man donating money to a charity and automatically evaluate him as kind and generous. We often consider, as best we can, his motives first. In this case, an attribution of a motive of personal gain for this act might lead us to temper our evaluative enthusiasm or even to assess him negatively as Machiavellian and untrustworthy. We might expect that such processes operate in our perception of language behaviour as well (cf Siebold, 1975). Indeed, implicit in our discussion of the perception of inconsistent others in the previous section was the notion of a 'search for causality'. That is, listeners would in all probability need to make sense of the unexpected speech input by attributing motives and intentions behind the apparent stimulus inconsistency. Therefore, different reasons attributed by a person for another's convergence, or even non-convergence, to him can seriously affect how the other is perceived and also the nature of the speech patterns which subsequently follow.

Simard, Taylor and Giles (1976) conducted an experiment with French Canadian students in Quebec who were prepared to receive a tape-recorded message from an English Canadian speaker. Half of the French Canadian listeners were converged to by the speaker (that is, he spoke in French) while the other half were not converged to (that is, he spoke in English). Overall, listeners reacted favourably to the speaker (and his message) when he converged towards them in French and rather negatively when he spoke to them in English. Besides the reasons already inherent in accommodation theory for predicting this result, Berger and Calabrese's (1975) model of initial encounters includes an attributional analysis of communicative behaviour which strengthens the theoretical assumptions. They state that when people communicate to others in a predictable fashion, this is attributed simply and with ease in a positive manner. On the other hand, when the manner of communication is rather unexpected, recipients find it difficult to attribute the intentions with any degree of confidence. Berger and Calabrese argue that this 'attribution problem' causes decoders cognitive discomfort which is reflected in a negative evaluation of the speaker. Given that people find satisfaction in being approved of by others, convergence may often be expected from speakers and as such is attributed with ease and positively. Non-convergence, however, may be regarded as more of an *un*expected strategy and as such is liable to be an attributional problem for the recipient leading to a negative evaluation of the encoder.

However, although interpersonal convergence in bilingual settings is generally very favourably received, and non-convergence generally unfavourably received, the extent to which this holds true will be influenced by the *specific* attribution of the speaker's intent. In the Simard *et al* study, different groups of French Canadian listeners were given different reasons why the speaker converged or did not converge towards them and this had the effect of modifying their evaluations of him. For instance, it was found

that when listeners attributed the convergence as due to a desire to break down cultural barriers, the shift was viewed very favourably. Yet when this same speech shift was attributed instead to pressures in the situation forcing the other to converge, his *true* intentions could not be so readily assessed and positive feelings were somewhat attenuated. Similarly, it was found that when non-convergence (the use of English by the English Canadian) was attributed to situational pressures demanding own-group language from the speaker, or to the fact that he was not bilingual anyway, negative attitudes were not so pronounced due to the extenuating circumstances as when his language maintenance was attributed to a lack of effort on his part. Simard *et al* argued that when there is no objective information as to how to attribute another's non-convergence, recipients might be more disposed towards considering that the speaker does not have the necessary sociolinguistic skills rather than attributing it to a lack of effort or interest in them. In this way, listeners can avoid the debilitating cognition that their respect or approval is not worthy of securing.

Thus, how we react to others who converge or do not converge towards us can be influenced by our attributions of the intent behind the speech act. The situation may become more complex in contexts where one is interacting with a member of another group which is in direct conflict with one's own. In cases of such intergroup tension, where valid and reliable information is often lacking about another's true intentions, attribution errors may arise due to unfavourable stereotypes held of outgroup members (Johnson, Feigenbaum and Weibey, 1964; Taylor and Jaggi, 1974). For instance, we may be more ready to attribute speech convergence by outgroup members to unknown pressures in the situation, or to deviousness, than to acknowledge a sincere desire on their part to reduce tension. In the same way, we may be more ready to attribute their non-convergence to a lack of effort rather than consider the possibility of their not possessing the necessary linguistic skills, or that there were strong cultural pressures forcing them to use their native speech style.

A real life example of this latter situation was provided by an American friend who went to study in Montreal in the early 1970s. On arrival in Quebec, his car bore licence plates from his home State. When he drove to a petrol station to fill up, he was more often than not greeted by attendants in French. Unfortunately, he was monolingual and could not converge back in reply, and so spoke in English. Extenuating circumstances for this non-convergence were apparent from his licence plates, that is, he was considered to be an American visitor, so the French Canadian attendants would converge back to him in English, and he would be served. After a few months, however, he was obliged by provincial legislation to display Quebec licence plates. Thereafter, whenever he asked for petrol from these same attendants - who, of course, could not recall him - their response was of the order 'ne comprends pas, monsieur' and he would experience great difficulty in acquiring petrol at all. In this situation, the French Canadians could attribute no extenuating circumstances for this apparent English Canadian (given the licence plates) not to converge to them in a bilingual city that was predominantly Francophone.

In this section, we have tended to discuss language convergence and non-convergence as if they were simply binary sociolinguistic choices speakers make depending upon their definition of the interactive context. The situation is, however, far more complex given that a speaker may converge on a variety of linguistic dimensions separately or in combination. Indeed, Giles, Taylor and Bourhis (1973) found in an analysis of speech in a bilingual context that an English Canadian speaker could converge towards a French Canadian listener in at least 14 different ways - even in the socially-sterile atmosphere of a laboratory setting. For example, some speakers would shift totally into French, others would use a mixture of both French and English, others would speak in English but would translate certain key words and concepts into French, while still others would remain totally in English but deliberately slow down their speech rate. The authors suggested that different types of convergence may be tentatively placed along a continuum of perceived effort where both speaker and listener might construe a given linguistic strategy as involving high, medium or low social concessions. This complex state of affairs leads us to pose a number of questions. For instance, are there any other individual difference or contextual factors which would affect the amount of convergence encoded? Are there some linguistic features of convergence evaluatively more effective than others? Are there optimal levels of convergence? Let us briefly consider each of these issues in turn.

Encoding convergence: individual difference and developmental variables

It is likely that a whole host of individual difference variables could affect the magnitude of convergence produced by a speaker besides the extent of his or her desire for social approval from the interlocutor. We will, however, discuss just a few examples here including the speaker's cognitive and perceptual styles. Witkin *et al* (1962) have shown that styles of approach to perceived surroundings differ among individuals and have described a style called 'field independence'; a highly analytic approach to perceived materials. This style in visual and auditory perception is indicated among individuals who can maintain attention to particular aspects of a perceived world no matter how confusing or 'embedding' are the surroundings; such people were also considered to perceive the self as distinct or more differentiated from the surrounding *social* relations. Wober (1967) comments that, 'they are likely to formulate their social contacts through the device of being able to take up several distinct roles, each of which can be switched into and out of at relevant times' (p 352). Support for the relationship between cognitive style and speech convergence has been found by Marcus, Welkowitz, Feldstein and Jaffe (1970) who showed that the convergence of silence durations in a dyad was more apparent, the more field independent the speakers.

It could be argued then that the more speakers are sensitive to cues in interpersonal situations, the more likely it is that they will converge (cf Taylor, Catford, Guiora and Lane, 1969; Welkowitz, Rothstein and Feldstein, 1973). In this sense, we would expect that since this emphatic capacity develops as the maturing child is more and more able to take account of listener characteristics (Piaget,

1955; Sunshine and Horowitz, 1968; Flavell *et al*, 1968; Krauss and Glucksberg, 1969), the potential for convergence will emerge concomitantly. This hypothesis is perhaps too simplistic as a study by Welkowitz, Cariffe and Feldstein (1976) suggests that different types of pause length convergences appear at different ages. Indeed, the facility to converge towards others with regard to certain linguistic features appears very early on in life (Giles and Powesland, 1975: 139-140; Dale, 1972: 278-279). Lieberman (1967), for instance, has observed children of about 12 months of age converging to the pitch patterns of their parents by lowering the fundamental frequency of their babbling in the presence of their father, and raising it with the mother. Nevertheless, Welkowitz, Cariffe and Feldstein (1975) showed that convergence of temporal aspects of speech is age-related. The developmental pattern of different forms of speech convergence is certainly an area worthy of study.

It is important to point out that while a desire for another's social approval is a prerequisite for convergence to take place, a sufficiently well-developed empathic capacity seems needed in order to facilitate the process.

Encoded convergence: contextual variables

It could be argued that when the purpose of an interaction for participants is more concerned with the development of affective interpersonal relations (a socio-emotional orientation) rather than with exchanging specific information and ideas (a task-related orientation), convergence will occur to a greater extent (cf Bales and Slater, 1955; Halliday, 1970). It is, however, difficult to categorize most social contexts dichotomously in this fashion. Nevertheless, the goals of various stages of an interaction often vacillate in one direction and then the other making it a useful distinction to explore linguistically (Fielding and Fraser, 1978). Moreover, besides the knowledge that convergence becomes greater each time members of a dyad meet (Lennard and Bernstein, 1960; Welkowitz and Feldstein, 1969), we have little idea of its role during the course of an encounter. It could be suggested that once members of a dyad have converged towards each other ultimately to their full potential, other linguistic strategies such as the use of humour and a more informal, verbal style (Fielding and Fraser, 1978) could be introduced to take its function of securing approval from the other (Giles, Bourhis, Gadfield, Davies and Davies, 1976). Another such strategy might be the introduction of *speech innovations* at the lexical, grammatical, phonological and non-verbal levels such as neologisms, peculiar intonation patterns etc, the meaning and use of which is apparent only to the interacting pair. The creation of such 'ingroup language' by one participant of a dyad may signal to the other that he considers a shared understanding has been achieved and that a rewarding relationship with its own unique characteristics may ensue. In this sense, families often adopt their own linguistic styles (Bossard, 1955; Read, 1962). Perhaps Berger's (1979) model of the development of interpersonal relationships can be utilized to crystallize these notions further (cf Clarke, 1975; Duck, 1977).

Decoding convergence: differential effectiveness of strategies

Although it is intuitively obvious that we can change several aspects of our style of speech simultaneously, the explicit comparison of the effectiveness of a speaker's convergence on one set of linguistic features with convergence at others has received only scant attention empirically. Among the more obvious intralingual convergences that could take place would be those of pronunciation, speech rate and message content. That is, people can attempt to make themselves more similar and intelligible to others by attenuating their distinctive accents, slowing down their speech rates, and by presenting the substance of their message in a manner that would take account of their listener's familiarity with the topic under discussion.

In a study by Giles and Smith (1979), a Canadian male tape-recorded eight versions of the same short message on Canadian education for an English audience in Britain. One of these, a non-converging version, was a message where the speaker did not converge towards the audience from a standard version supposedly spoken to Canadians back home on any of the three speech variables mentioned above. The remaining seven versions represented all possible combinations of pronunciation, speech rate, and content convergence - non-convergence. Although listeners appreciated the speaker's convergence on all three linguistic levels, it was found that they upgraded him in attractiveness more when he converged in speech rate than either pronunciation or content. Interestingly, a number of writers (see, for example, in Siegman and Feldstein, 1978) have argued for the fundamental importance of *temporal* aspects of conversational features in an interpretation of dyadic encounters. However, we have gained only rudimentary knowledge about what constitutes an effective convergent strategy from the perspective of listeners, as it is undoubtedly the case that the salience of a particular feature converged will depend on the context. For instance, had the stimulus speaker in this study represented an outgroup which had a low social and accent prestige, or was in conflict with the listeners' own group, pronunciation convergence might have been perceived as more of a concession to the audience and had a correspondingly more positive effect, given that this feature is often the marker of group membership (Bourhis, Giles and Tajfel, 1973; Ryan and Carranza, 1977).

Decoding convergence: optimal levels

As discussed thus far, accommodation theory would suggest that the more a speaker converged towards another the more positively he would be evaluated by the latter. Therefore, a speaker who converged on all three of the linguistic features mentioned above should be more highly evaluated than one who converged on only one or two of them. However, this ignores the possibility that large increases in convergence may not be attributed to a positive intent on behalf of the speaker, but seen instead as patronizing, condescending, threatening or ingratiating (cf Jones and Jones, 1964). For instance, one could imagine an American visitor in the presence of British listeners, adopting what he considered to be a typically English mode

of self-presentation. If the convergent shifts were very marked, the audience might be disposed to think that the American considered them (from his accent) in terms of an outdated stereotype, and (from the content of his message) grossly ignorant about certain commonly-debated matters. Similarly, Fanon (1961) has talked of the patronizing speech Whites sometimes adopt with Blacks, making them feel that they have been considered childlike or even sub-human (cf Shuy, 1977; Lukens, 1979).

Another concern of the Giles and Smith (1979) study was to determine whether a speaker is most positively assessed when making the most convergence. It was found that in fact a non-linear relationship between convergence and attraction held. The most positively evaluated convergent strategy of the eight sampled by listeners was the combination of content plus speech rate convergences; the effect of adding pronunciation (to this *optimum*) was to attenuate the favourableness of listeners' perceptions of the speaker. According to listeners' ratings, they thought that the speaker had an uncomplimentary view of them when he converged on all three linguistic features. The addition of pronunciation (in *this* instance) shifts could have been seen as patronizing, ingratiating or as a caricature. Alternatively, or in combination, pronunciation shifts plus the other convergences (called 'over-accommodation') may have been seen as threatening. English listeners may have felt that they were losing their cultural distinctiveness as the Canadian adopted perhaps the most distinguishing linguistic attribute of their group membership - a 'British' accent (cf later section on Tajfel's theory). In this way, the speaker may have been perceived as stripping them of the veil of their group distinctiveness while, at the same time, displaying how easy it was to take account of their characteristics by content and speech rate convergences.

It can be argued that listeners may have a *tolerance* for certain amounts of convergence, and hence a move beyond a certain threshold (which may vary situationally) may be negatively perceived by them (cf Read, 1964; Sherif, Sherif and Nebergall, 1965; Karam, 1976). Moreover, it might be interesting to explore notions of what we might call 'linguistic equilibria'. Argyle and Dean (1965) showed that in face-to-face interactions an equilibrium of non-verbal behaviours was apparent in initial encounters. For instance, when people are in very close proximity a minimal amount of eye contact is tolerated. However, as the spatial distance between interactants is increased, so too is their eye contact. Argyle and Dean argue that in some cultures the level of intimacy must be controlled in first encounters and that too much of it can be detrimental to the smooth running of an interaction. Sociolinguistically, too, speakers may attempt to attain an equilibrium of linguistic features in their convergence, the intensity of one being balanced by the lack of strength of others. In further research, it would be interesting to determine whether optimal levels of convergence in different social situations (eg between sex and intergenerational encounters) have the same linguistic features in common. It is our guess that they will not and furthermore that the underlying reasons for the negative attribution towards over-accommodation might be different.

Besides optimal magnitudes of convergence, Giles and Smith speculated that there might also be optimal *rates* of convergence. Aronson's (1972) 'gain-loss' theory of attraction proposes that we like more those people whose respect we are acquiring rather than those whose admiration we already possess. It could then be suggested that convergence is more effective when it takes place slowly enough so that the change is perceived by degrees, rather than all at once (cf Altman and Taylor, 1973). The latter strategy might be costly for the speaker making him vulnerable to the inference that his respect was transparent and secure from the outset. As may be recalled from earlier discussion, people naturally converge more to each other on subsequent occasions, appearing to conserve convergent acts for use as bargaining tools or 'aces-in-the-hole', as gain-loss theory would predict. In this sense, Giles and Smith proposed that speech convergence might possess a 'negotiative' character. However, our perspective of optimal convergence has been rather static in the sense that the decoding of over-accommodation has been discussed without reference to the recipient's subsequent *action*. For instance, would the latter reply in such a manner as to induce the other to relocate his convergence more optimally?

DIVERGENCE

Thus far, we have considered convergence as an active process while non-convergence has assumed a more passive, subordinate role considered only from the perspective of the decoder. This latter orientation is unfounded particularly given that non-convergence, or speech maintenance (Bourhis, 1977), can be used by ethnic groups as a symbolic tactic for maintaining their identity and cultural distinctiveness. This was exemplified a little while ago when, for the first time, the Arab nations issued their oil communiqué to the world not in English but in Arabic instead. Likewise one witnesses the efforts of many ethnic minorities throughout the world attempting to maintain their own dialects as an expression of cultural pride such as the Québécois, Welsh, Basques and Catalans (Fishman, 1966; Giles, 1977b). Moreover, it may well be that under certain conditions people not only want to maintain their own speech style, but wish to *emphasize* it in interaction with others. In such cases, speakers want to accentuate the difference between themselves and others (Wolff, 1959; Tajfel and Wilkes, 1963; Lambert and Lambert, 1972) perhaps because of the others' outgroup membership, undesirable attitudes, habits or appearance. This process of social dissociation has been termed 'speech divergence' (Giles, 1973a), and is the opposite of convergence in that it involves speakers modifying their speech away from their interlocutors and increasing the communicative distance between them (Peng, 1974). In many situations the recipient of divergence is likely to react negatively to this tactic, although sympathetic confederates of the diverging speaker might find it laudable on some dimensions (Bourhis, Giles and Lambert, 1975; Doise, Sinclair and Bourhis, 1976; Bourhis, 1977). If both participants of a dyad are similarly dissociatively motivated then they may be symmetrical in their efforts towards progressive divergence to an optimally effective level (Giles and Smith, 1979). It can be argued, however, that divergence may be an infrequent speech modification because of the common tendency people have in attempting to

avoid, or at least to minimize, interactions with those with whom they desire little affiliation; in this way the need for frequent divergence is reduced.

It is worthwhile pointing out that divergence may occur not only by simple dissociation away from the interlocutor towards an opposing reference group, but also by expressing sociolinguistically a greater identification with that other's reference group than the other can display himself (Giles, 1971b; Kelly, 1975). For example, when talking to a shop assistant who is using a higher prestige language variety than one's own in a seemingly aloof manner, one might adopt an even more prestigious speech style than he in order to display one's greater qualifications to appeal in this direction anyway. Similarly, when talking to an old school-friend who is using a lower prestige code than you while chiding your apparent aloofness, one might adopt a more basilect code than even he in order to show one's greater identification with local values. These strategies may be called upward and downward *cross-over* divergence respectively.

Giles and Powesland (1975) argue that both speech convergence and divergence may be seen as representing strategies of conformity and identification. Speech convergence is a strategy of identification with the speech patterns of an individual internal to the social interaction, whereas speech divergence may be regarded as a strategy of identification with regard to the linguistic norms of some reference group *external* to the immediate situation. To the extent that divergent strategies are adopted probably more often in dyads where the participants derive from different social or ethnic backgrounds, the incorporation of ideas from Tajfel's theory of intergroup relations and social change provides an appropriate context within which to consider divergent shifts more generally (Giles, Bourhis and Taylor, 1977).

Tajfel's theory of intergroup processes

Tajfel (1974) has suggested that when members of a group interact with members of another, they compare themselves on dimensions which are important to them, such as personal attributes, abilities, material possessions and so forth. He suggests that these 'intergroup social comparisons' will lead individuals to search for, or even to create, dimensions on which they may be seen to be positively distinct from a relevant outgroup. The perception of such a positive distinctiveness by the ingroup will contribute to their feelings of an adequate social identity. In other words, people experience satisfaction in the knowledge that they belong to groups which enjoy some superiority over others. Given that speech style is for many people an important subjective dimension of, and objective clue to, social and particularly ethnic group membership (Taylor, Bassili and Aboud, 1973; Giles, Taylor, Lambert and Albert, 1976; Fishman, 1977), it has been proposed that in certain intergroup encounters, individuals might search for a positively-valued distinctiveness from an outgroup member on linguistic dimensions they value highly; a process termed 'psycholinguistic distinctiveness' (Giles, Bourhis and Taylor, 1977). A study by

Parkin (1977) can be used to demonstrate this at a putative level. He found that members of adolescent societies and gangs in Nairobi felt a need to make themselves distinct from each other by a *claimed* use of English and Swahili respectively, even though their language behaviour appeared objectively very similar. However, perhaps the example *par excellence* of psycholinguistic distinctiveness at an objective behavioural level, is speech divergence. By diverging and emphasizing their own social or ethnic accent, dialect or language, members of an ingroup would accentuate the differences between themselves and the outgroup on a salient and valued dimension of their group identity.

Bourhis and Giles (1977) designed an experiment to demonstrate the use of *accent* divergence among Welsh people in an inter-ethnic context, and to investigate the conditions which would facilitate its occurrence. The study was conducted in a language laboratory where people who valued their national group membership and its language highly were learning the Welsh language; only 26% of Welshmen can speak their national tongue. During one of their weekly sessions, Welshmen were asked to help in a survey concerned with second language learning techniques. The questions in the survey were verbally presented to them in English in their individual booths by a very English-sounding speaker who at one point arrogantly challenged their reasons for learning what he called 'a dying language with a dismal future'. Such a question was perceived by them to threaten their feelings of ethnic identity, and the informants broadened their Welsh accents in their replies as compared with their answers to a previously-asked, emotionally-neutral question. In addition, some informants introduced Welsh words and phrases in their answers, while one woman did not reply for a while and then was heard to conjugate Welsh verbs very gently into the microphone. Interestingly, even when asked a neutral question beforehand, informants emphasized their Welsh group membership to the English speaker in terms of the content of what they said to him in their replies (termed 'content differentiation'), demonstrating that psycholinguistic distinctiveness can occur in many different forms.

The notion of nominal-verbal styles may also be useful in this last respect. Fielding and Fraser (1978) have argued that formal speech is characterized by a nominal style which contains long sentences with low structural diversity, many articles, prepositions, adjectives and nouns and is subjectively viewed as impersonal, pedestrian and monotonous. Informal speech, on the other hand, is characterized by a verbal style which contains shorter sentences with a rich structural diversity, many pronouns, adverbs and verbs and is viewed as personal and vivid. The authors claim that the nominal style 'is a consequence of, and marks the speaker's dislike of her listener, and indicates her wish to distance herself'. We may speculate that accent divergence may also be accompanied by an increase in the grammatical indices of a nominal style. Moreover, it may well be that there is a hierarchy of strategies of psycholinguistic distinctiveness, some being more symbolic of social dissociation than others. Perhaps both from the perspectives of ingroup encoder and outgroup decoder, putative, a few pronunciation and content differentiations may be considered as increasingly more

intense, but nevertheless instances of low level psycholinguistic distinctiveness. On the other hand, various forms of accent and dialect (including nominal-verbal style) divergence may be considered instances of stronger social dissociation. Verbal abuse, the maintenance of or switch to another language in the face of an outgroup speaker, may be among the most potent forms of psycholinguistic distinctiveness given their extremely overt, dissociative characters (cf Lukens, 1979).

This notion of *language* divergence was investigated in Belgium by Bourhis, Giles, Leyens and Tajfel (1979). Their study involved different groups of trilingual Flemish students (Flemish-French-English) being recorded in 'neutral' and 'ethnically threatening' encounters with a Walloon (Francophone) outgroup speaker. As in the Bourhis and Giles study, the context of the interaction was a language laboratory, where participants were attending classes to improve their English skills. Many Flemish and Francophone students converse together in English, it being an emotionally neutral compromise between maintaining rigid linguistic distinctiveness, and acquiescing to pressures to converge using the other's language. In this experiment, the speaker spoke to the students in English although indicating himself as a Walloon by means of distinctive Francophone phonological realizations in that language. It was found that when the speaker demeaned the Flemish in his ethnically threatening question, listeners rated him as sounding more Francophone (a process termed *perceptual* divergence), and themselves as feeling more Flemish, than when a neutral question was asked. This cognitive dissociation was manifest behaviourally at a covert level by means of muttered or whispered disapproval while the Walloon was speaking (tape-recorded unknowingly to the informants), and at an overt level through divergent shifts to own-group language (Flemish). However, this divergence only occurred under certain specific, experimental conditions, and then for only 50% of the sample. It was found that only when their own group membership and that of the speaker's was emphasized by the investigator (ie, when intergroup categorization was made salient), and when the speaker had been known from the outset to be antithetical to Flemish ethnolinguistic goals, did language divergence emerge. In a follow-up study, however, language divergence into Flemish did occur for nearly 100% of the informants under these same conditions, but only when the Walloon speaker himself diverged into French in his threatening question.

Interestingly, the form of the language divergence in the first of these Belgian studies differed from that in the second. It was found that, in the former, the ingroup replied to the outgroup threat first in English and then switched into Flemish. In the latter (more threatening situation), listeners replied in a directly divergent manner by a complete shift into Flemish. Language divergence then, like language convergence (Giles, Taylor and Bourhis, 1973), can take on many forms and may, of course, be influenced by a number of factors not discussed here, including the perceived legitimacy-illegitimacy of the intergroup status positions for ingroup members (Turner and Brown, 1979).

Speech style divergence can, therefore, be used as a tactic to maximize differences between groups on a valued dimension in the search for a positive distinctiveness. The dynamic flux of relationships between dominant and subordinate groups, where one strives to assume distinctiveness over the other, undoubtedly is reflected in their speech behaviour and, as such, might be a potentially important dimension to explore for an understanding of linguistic change (cf Peng, 1976; Littlewood, 1977; Ros and Giles, 1979). It is important to stress, however, that psycholinguistic distinctiveness *vis à vis* a competing outgroup does not in itself mean that a group has achieved a satisfactory social identity; this might be particularly true in situations where economic and power disparities still exist between in- and outgroup. This is a case where direct group competition may be the only way eventually to restore a group's social identity (cf Turner, 1975; Tajfel and Turner, 1979; Giles, Bourhis and Taylor, 1977).

CONVERGENCE, DIVERGENCE AND COMPLEMENTARITY

We have pointed out that convergence (and also divergence) can be manifest on more than one dimension simultaneously, and that listeners can identify levels of these (Giles, 1973a; Giles and Smith, 1979; Bourhis and Giles, 1977). This raises the possibility that a speaker might, with a mind to being intentionally ambiguous, attempt to converge with regard to one linguistic feature (speech rate for example), while not converging, or even diverging on another (eg pronunciation). An example of this can be found in Montréal where on occasions French Canadian shoppers have been known to address Anglophone store assistants in fluent English, while requesting the services of a Francophone assistant instead. This is an instance of convergence in terms of linguistic form, but psychologically the message content is, of course, one of social dissociation and divergence. The effect might be even more subtle than intimated here if one considers (as do Bourhis and Giles, 1977) that shifts in speech style can occur on two dimensions simultaneously - one for the benefit of the outgroup and another (not even perceived by the outgroup) for the benefit of members of the ingroup present as a covert expression of solidarity and defiance. Holt (1973) provides us with an example of this phenomenon, namely 'linguistic inversion', engaged in by Black slaves in the United States in the last century. The meaning of many phrases and words (eg 'nigger') when said to a White meant something quite different and even positive among the ingroup than the outgroup would ever have taken it for. In fact, Blacks often engaged in what would seem to Whites to be overtly convergent phrases that for other Blacks in the situation would indicate covert divergence.

In contrast, however, simultaneous shifts away from and towards the other in a dyad can occur in ways that can be regarded as totally integrative for both participants. Let us introduce a distinction between symmetrical and complementary relationships (Watzlawick, Beavin and Jackson, 1967). Symmetry in a relationship refers to an egalitarian situation obtaining in a dyad when both participants consider themselves equal in all relevant respects. A complementary relationship obtains when one participant is acknowledged to hold

a subordinate role to the other's one-up position. Many examples of this can be given where a status or power discrepancy exists in a dyad, including doctor-patient, employer-employee, teacher-pupil interactions as well as a traditional male-female situation. However, not all relationships can be classified in these categorical terms and often stable relationships (eg husband and wife) veer in a symmetrical and then in a complementary direction depending on the nature of the topic discussed or the situation involved. It is important to emphasize that complementary relationships do not abound without the consensus of the participants involved, and that complementarity increases mutual predictability as argued speech convergence would earlier (Berger and Calabrese, 1975). Miller and Steinberg (1975: 235) argue that

> Many people do indeed seem to choose to be one-down in their relationships with others; they consistently adopt subservient, deferential or even totally dependent positions. In doing so, they are able to achieve some measure of certainty in their communication transactions. Their consistently one-down behaviour tends to elicit predictably one-up kinds of responses from their companions. In this sense, any role is preferable to a variable one, or to no role at all.

The acceptance of this role differentiation by the interlocutors will, of course, be manifest behaviourally. Someone entering an interaction where they accept a subordinate role will signal this by modifying their speech patterns in a complementary way to that of the other. Such speech shifts shall be called instances of 'speech complementarity' and can be regarded as divergence in a simple descriptive linguistic sense, yet psychologically involve acceptance of the situation rather than dissociation. Classic examples of speech complementarity might be when two young people are out on a date. Each accentuate their respective masculine and feminine qualities by means of linguistic as well as non-linguistic strategies. This does not, however, preclude the possibility of convergence simultaneously occurring on other linguistic dimensions. For instance, a woman may adopt a soft voice and certain paralinguistic and intonational features with an eligible bachelor lawyer, yet may wish to gain his attraction, approval and respect by not only fulfilling her feminine role requirements, but also by converging to his higher prestige dialect. Let us also recall our previous example of a job applicant being interviewed by his prospective employer. Although the former is likely to be perceived favourably because of his convergent strategies, had he failed simultaneously to maintain his inferior role position by means of other verbal and non-verbal cues, his overall performance may have been evaluated very poorly.

Future work will determine whether certain standard linguistic features are used as vehicles for speech complementarity, whereas certain others are consistently adopted for convergence strategies trans-situationally. Again, it is likely that there are optimal levels of speech complementarity. Miller and Steinberg (1975: 239-240) provide an illustration of this:

> When the scene shifts to social activities with the husband's professional associates, the wife assumes the submissive one-down position .... He, in turn, refrains from exploiting the situation: he does not become unduly dominant or unreasonable in his demands for he knows that at a certain point his wife will be forced to defect from her one-down position.

Speech convergence is then often accompanied by speech complementarity of other linguistic features. Naturally, the optimal degree and rate of convergence, together with the optimal balance of the appropriate amount of complementarity, is, of course, difficult to encode from situation to situation. As Goffman (1967) says, 'the image that emerges of the individual is that of juggler and synthesizer, an accommodator and appeaser, who fulfils one function while he is apparently engaged in another'. Seen in this light, it is no wonder that the effectiveness of interpersonal communication is often wrought with difficulties and misunderstandings. Moreover, it is possible that atypical uses of speech accommodative strategies by speakers could in certain instances contribute to the attribution of their psychological disorder and mental ill-health by others.

CONCLUSION

Accommodation theory is then concerned with explaining some of the processes underlying convergence, divergence and complementarity. It is, of course, still a theory in development and cannot explain all interpersonal speech shifts in all verbal and cultural contexts. Even in the case of approval-seeking, the theory presents some difficulties in its applications to some real life situations. Hymes (pers. commun.) has provided an example of this by citing a case of an English-speaking European addressing an East African official. In order to converge, the European begins speaking in Swahili. According to the theory, this should show a desire for solidarity and should gain the official's approval. In this context, however, such convergence would probably be interpreted by the official as condescension and as implying that the European thought him incapable of understanding English. It would, therefore, be regarded as insulting. Hymes suggests that the appropriate tactic would be for the European to use English first, allowing the official to demonstrate his linguistic competence by replying in that language, and then to continue in Swahili. However, further empirical explorations within the framework of accommodation theory may allow us to make more precise statements about when convergence, divergence and complementarity are operative, what factors influence the extent of their occurrence, what their optimally-effective levels might be, and what the role of awareness is in these processes from both the perspective of encoder and decoder. This chapter has been concerned with expanding some of the empirical and theoretical limits of the theory.

Of all its shortcomings, perhaps the theory's greatest is its lack of linguistic sophistication and input. We have little knowledge about the specific linguistic features speakers converge towards when they wish another's approval (Beebe, 1976), when they

diverge away from another in order to dissociate (Segalowitz and Gatbonton, 1977), or when they complement another so as to reinforce an expected role differentiation. Moreover, although convergence, divergence and complementarity may be universal phenomena, their most salient linguistic manifestations may vary cross-culturally. Indeed, Scherer (1974, 1979) has evidence suggesting that the voice qualities which differentiate speakers from amongst each other in the United States, are not the same as those in Germany, and that vocal features of a speaker considered to be socially influential in the two countries also differ. However, this limitation of the theory to a socio-bias may arguably be *legitimate* given that most previous sociolinguistic formulations have been loaded more in a linguistic direction (Smith, Giles and Hewstone, 1980). Accommodation theory admittedly grew out of a desire to demonstrate the value and potential of social psychological phenomena and theory for increasing our understanding of the dynamics of language in a social context; it is hoped that this chapter has been persuasive in this respect. Nevertheless, as argued elsewhere, if the language sciences in general, and sociolinguistics in particular, are going to develop a methodology and complete theory of speech behaviour, it is going to have to be a multidisciplinary one. Robert Le Page has always argued for such an eclectic orientation and it is high time that it was more generally attempted. Accommodation theory, which is receptive to the introduction of greater linguistic specifications, may provide a useful means for doing this.

FOOTNOTES

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2. Cf Argyle, 1969, Matarazzo, 1973, Jaffe and Feldstein, 1972, Welkowitz and Kuc, 1973, and Natalé, 1975a for somewhat similar explanations of this phenomenon.

REFERENCES

Aboud, F E, R Clément and D M Taylor
1974 'Evaluational reactions to discrepancies between social class and language', Sociometry 37: 239-50.

Altman, I and D A Taylor
1973 Social Penetration: The Development of Interpersonal Relationships, New York: Holt, Rinehart and Winston.

Argyle, M
1969 Social Interaction, London: Methuen.

Argyle, M and J Dean
1965 'Eye-contact, distance and affiliation', Sociometry 28: 289-304.

Aronson, E
1972 The Social Animal, New York: Freeman.

Bales, R F and P E Slater
1955 'Role differentiation in small decision-making groups', in T Parsons and R F Bales (ed.) Family, Socialisation and Interaction Process, New York: Free Press, 259-306.

Beebe, L M
1976 'The influence of the listener on code-switching'. Paper delivered at LSA Summer Meeting, Oswego, New York State; Teachers College, Columbia University, New York City (mimeo).

Berger, C R
1979 'Beyond initial interaction: Uncertainty, understanding, and the development of interpersonal relationships', in H Giles and R St Clair (ed.) Introducing Language and Social Psychology, Oxford: Blackwell, 122-44.

Berger, C R and R J Calabrese
1975 'Some explorations in initial interaction and beyond: Toward a developmental theory of interpersonal communication', Human Communication Research 1: 99-112.

Berger, C R, M D Weber, M E Munley and J T Dixon
1977 'Interpersonal relationship levels and interpersonal attraction', Communication Studies, Northwestern University, Evanston, Ill. (mimeo).

Bossard, J H S
1955 'Family modes of expression', American Sociological Review 10: 226- 37.

Bourhis, R Y
1977 Ph D dissertation, University of Bristol.

Bourhis, R Y and H Giles
1976 'The language of cooperation in Wales: A field study', Language Sciences 42: 13-16.

Bourhis, R Y and H Giles
1977 'The language of intergroup distinctiveness', in H Giles (ed.) Language, Ethnicity and Intergroup Relations (European Monographs in Social Psychology), London and New York: Academic Press, 119-36.

Bourhis, R Y, H Giles and W E Lambert
1975 'Social consequences of accommodating one's style of speech: A cross-national investigation', International Journal of the Sociology of Language 6: 55-72.

Bourhis R Y, H Giles, J P Leyens and H Tajfel
1979 'Psycholinguistic distinctiveness: Language divergence in Belgium', in H Giles and R St Clair (ed.) Introducing Language and Social Psychology, Oxford: Blackwell, 158-85.

Bourhis R Y, H Giles and H Tajfel
1973 'Language as a determinant of Welsh identity', European Journal of Social Psychology 3: 447-60.

Bradac J J, J A Courtright, G Schmidt and R A Davies
1976 'The effects of perceived status and linguistic diversity upon judgements of speaker attributes and message effectiveness', Journal of Psychology 93: 213- 20.

Byrne, D
1969 'Attitudes and attraction', Advances in Experimental Psychology 4: 35-89.

Clarke, D D
1975 'The use and recognition of sequential structure in dialogue', British Journal of Social and Clinical Psychology 14: 333- 40.

Courtney, A E and J W Whipple
1974 'Woman in TV commercials', Journal of Communication 24: 110- 18.

Dale, P S
1972 Language Development: Structure and Function, Hinsdale, Ill: Dryden Press.

Doise, W, A Sinclair and R Y Bourhis
1976 'Evaluation of accent convergence and divergence in cooperative and competitive intergroup situations', British Journal of Social and Clinical Psychology 15: 247-52.

Duck, S (ed.)
1977 Theory and Practice in Interpersonal Attraction, London and New York: Academic Press.

Elyan, O, P M Smith, H Giles and R Bourhis
1978 'RP-accented female speech: The voice of Androgyny?', in P Trudgill (ed.) Sociolinguistic Patterns in British English, London: Arnold, 122-39.

Ervin-Tripp, S M
1969 'Sociolinguistics', Advances in Experimental Social Psychology 4: 91-165.

Fanon, F
1961 Black Skin, White Masks, New York: Grove Press.

Feldman, R E
1968 'Response to compatriots and foreigners who seek assistance', Journal of Personality and Social Psychology 10: 202- 14.

Feldstein, S
1972 'Temporal patterns of dialogue: Basic research and reconsiderations', in A W Siegman and B Pope (ed.) Studies in Dyadic Communication, New York: Pergamon.

Fielding, G and C Fraser
1978 'Language and interpersonal relations', in I Markova (ed) Language in its Social Context, London: Wiley.

Fishman, J A
1977 'Language and ethnicity', in H Giles (ed) Language, Ethnicity and Intergroup Relations (European Monographs in Social Psychology), London and New York: Academic Press, 15-58.

Fishman, J A *et al*
1966 Language Loyalty in the United States, The Hague: Mouton.

Flavell, J H, P T Botkin, C L Fry Jr, J W Wright and P E Jarvis
1968 The Development of Role-Taking and Communication Skills in Children, New York: Wiley.

Gardner, R C
1979 'Social psychological aspects of second language acquisition', in H Giles and R St Clair (ed.) Introducing Language and Social Psychology, Oxford: Blackwell, 193-220.

Giles, H
1971a 'Patterns of evaluation in reactions to RP, South Welsh and Somerset accented speech', British Journal of Social and Clinical Psychology 10: 280- 81.

1971b A study of speech patterns in social interaction: Accent evaluation and accent change. Ph D dissertation, University of Bristol.

1973a 'Accent mobility: A model and some data', Anthropological Linguistics 15: 87-105.

1973b 'Communicative effectiveness as a function of accented speech', Speech Monographs 40: 330- 31.

1977a 'Social psychology and applied linguistics: Towards an integrative approach', ITL: A Review of Applied Linguistics 33: 27-42.

Giles, H (ed.)
1977b Language, Ethnicity and Intergroup Relations (European Monographs in Social Psychology), London and New York: Academic Press.

Giles, H, S Baker and G Fielding
1975 'Communication length as a behavioural index of accent prejudice', International Journal of the Sociology of Language 6: 73-81.

Giles, H and R Y Bourhis
1976 'Black speakers with white speech - a real problem?', in G Nickel (ed.) Proceedings of the 4th International Congress of Applied Linguistics, Stuttgart: U.P.

Giles, H, R Y Bourhis, N Gadfield, G Davies and A P Davies
1976 'Cognitive aspects of humour in social interaction: A model and some linguistic data', in A J Chapman and H C Foot (ed.) Humour and Laughter: Theory, Research and Applications, London: Wiley, 139-54.

Giles, H, R Y Bourhis and D M Taylor
1977 'Towards a theory of language in ethnic group relations', in H Giles (ed.) Language, Ethnicity and Intergroup Relations (European Monographs in Social Psychology), London and New York: Academic Press, 307-48.

Giles, H and P F Powesland
1975 Speech Style and Social Evaluation (European Monographs in Social Psychology), London and New York: Academic Press.

Giles, H and R St Clair (ed.)
1979 Introducing Language and Social Psychology, Oxford: Blackwell.

Giles, H and P M Smith
1979 'Accommodation theory: Optimal levels of convergence', in H Giles and R St Clair (ed.) Introducing Language and Social Psychology, Oxford: Blackwell, 45-65.

Giles, H, D M Taylor and R Y Bourhis
1973 'Towards a theory of interpersonal accommodation through speech: Some Canadian data', Language in Society 2: 177- 92.

Giles, H, D M Taylor, W E Lambert and G Albert
1976 'Dimensions of ethnic identity: An example from Northern Maine', Journal of Social Psychology 100: 11-19.

Goffman, I
1967 Presentation of Self in Everyday Life, Harmondsworth, Middlesex: Penguin.

Halliday, M A K
1970 'Language structure and language function', in J Lyons (ed.) New Horizons in Linguistics, Harmondsworth: Penguin, 140-65.

Harris, M B and H Baudin
1973 'The language of altruism: The effects of language, dress and ethnic group', Journal of Social Psychology 97: 37-41.

Heider, F
1958 The Psychology of Interpersonal Relations, New York: Wiley.

Herman, S
1961 'Explorations in the social psychology of language choice', Human Relations 14: 149-64.

Holt, G S
1973 'Inversion in black communication', in T Kochman (ed.) Rappin' and Stylin' Out: Communication in Urban Black America, Urbana-Champaign: Illinois U.P.

Homans, G C
1961 Social Behaviour, New York: Harcourt, Brace and World.

Hymes, D
1972 'Models of the interaction of language and social setting', in J J Gumperz and D Hymes (ed.) Directions in Sociolinguistics: The Ethnography of Communication, New York: Holt, Rinehart and Winston.

Jaffe, J and S Feldstein
1972 Rhythms of Dialogue, New York and London: Academic Press.

Johnson, T J, R Feigenbaum and M Weibey
1964 'Some determinants and consequences of the teacher's perception of casuality', Journal of Educational Psychology 55: 237-46.

Jones, C and E Aronson
1973 'Attribution of fault to a rape victim as a function of respectability of the victim', Journal of Personality and Social Psychology 26: 415-19.

Jones, E E and K E Davis
1965 'From acts to dispositions: The attribution process in perception', in L Berkowitz (ed.) Advances in Experimental Social Psychology, New York and London: Academic Press.

Jones, R G and E E Jones
1964 'Optimum conformity as an ingratiation tactic', Journal of Personality 32: 4-36.

Karam, F X
1976 'Process of increasing mutual intelligibility', Linguistics, University of Southern California, Los Angeles (mimeo).

Kelley, H H
1973 'The process of causal attribution', American Psychologist 28: 107-28.

Kelly, J
1975 'Social forces and linguistic variability in teenage subcultures'. Paper delivered at the 4th International Congress of Applied Linguistics, Stuttgart, September 1975; University of Newcastle-upon-Tyne (mimeo).

Koeske, G and W D Crano
1968 'The effect of congruous and incongruous source-statement combinations upon the judged credibility of a communication', Journal of Experimental Social Psychology 4: 384-99.

Kramer, C
1978 'Female and male perception of female and male speech', Language and Speech 20: 151-61.

Krauss, R M and S Glucksberg
1969 'The development of communicative competence as a function of age', Child Development 40: 255-66.

Labov, W
1966 The Social Stratification of English in New York City, Washington, DC: Center for Applied Linguistics.

Lambert, W E and R C Gardner
1972 Attitudes and Motivation in Second Language Learning, Rowley, Mass: Newbury House.

Lambert, W E and W W Lambert
1972 Social Psychology, Englewood Cliffs, NJ: Prentice-Hall.

Larsen, K, H Martin and H Giles
1977 'Anticipated social cost and interpersonal accommodation', Human Communications Research 3: 303-08.

Lennard, H L and A Bernstein
1960 'Interdependence of therapist and patient verbal behaviour', in J A Fishman (ed.) Readings in the Sociology of Language, The Hague: Mouton.

Le Page, R B
1968 'Problems of description in multilingual communities', Transactions of the Philological Society: 189-221.

Lieberman, P
1967 Intonation, Perception and Language, Cambridge, Mass: MIT Press.

Littlewood, W T
1977 'Linguistic change during interpersonal interaction', Lingua 41: 1-11.

Lukens, J
1979 'Interethnic conflict and communicative distance', in H Giles and B Saint-Jacques (ed.) Language and Ethnic Relations, Oxford: Pergamon, 143-58.

Marcus, E, J Welkowitz, S Feldstein and J Jaffe
1970 'Psychological differentiation and the congruence of temporal speech patterns'. Paper presented at the annual meeting of the Eastern Psychological Association, Atlantic City, NJ, April.

Matarazzo, J D
1973 'A speech interaction system', in D J Kiesler (ed.) The Process of Psychotherapy, Chicago: Aldine.

McAllister, A and D Kiesler
1975 'Interviewee disclosure as a function of interpersonal trust, task modelling and interviewer self-disclosure', Journal of Consulting and Clinical Psychology 43: 428.

McArthur, L Z and B G Resko
1975 'The portrayal of men and women in American television commercials', Journal of Social Psychology 97: 209-20.

Miller, G R and M Steinberg
1975 Between People: A New Analysis of Interpersonal Communication, Chicago: Science Research Associates.

Natalé, M
1975a 'Convergence of mean vocal intensity in dyadic communication as a function of social desirability', Journal of Personality and Social Psychology 32: 790-804.

1975b 'Social desirability as related to convergence of temporal speech patterns', Perceptual and Motor Skills 40: 827-30.

Parkin, D
1977 "Emergent and stabilized multilingualism: Polyethnic peer groups in urban Kenya', in H Giles (ed.) Language, Ethnicity and Intergroup Relations (European Monographs in Social Psychology), London and New York: Academic Press, 185-210.

Peng, F C C
1974 'Communicative distance', Language Sciences 31: 32-35.

1976 'A new explanation of language change: The sociolinguistic approach', Forum Linguisticum 1: 67-93.

Piaget, J
1955 The Language and Thought of the Child, New York: World.

Powesland, P F and H Giles
1975 'Persuasiveness and accent-message incompatibility', Human Relations 28: 85-93.

Price, R H and D L Bouffard
1974 'Behavioural appropriateness and situational constraint as dimensions of social behaviour', Journal of Personality and Social Psychology 30: 579-86.

Raffler Engel, W von
1980 'The unconscious element in inter-cultural communication', in R St Clair and H Giles (ed.) Social and Psychological Contexts of Language, Hillsdale, NJ: Embank.

Read, A W
1962 'Family words in English', American Speech 37: 5-12.

1964 'The splitting and coalescing of widespread languages', in Proceedings of 9th International Congress of Linguists, The Hague: Mouton, 1129-34.

Rokeach, M, P W Smith and R J Evans
1960 'Two kinds of prejudice or one?', in M Rokeach (ed.) The Open and Closed Mind, New York: Basic, 133-68.

Ros, M and H Giles
1979 'The language situation in Valencia: An accommodation framework', ITL: Review of Applied Linguistics 44: 3-24.

Ryan, E B and M A Carranza
1977 'Ingroup and outgroup reactions to Mexican American language varieties', in H Giles (ed.) Language, Ethnicity and Intergroup Relations (European Monographs in Social Psychology), London and New York: Academic Press, 59-82.

Schachter, S and J E Singer
1962 'Cognitive, social and physiological determinants of emotional state', Psychological Review 69: 121-28.

Scherer, K R
1974 'Voice quality analysis of American and German speakers', Journal of Psycholinguistic Research 3: 281-98.

1979 'Voice and speech correlates of perceived social influence in simulated juries', in H Giles and R St Clair (ed.) Introducing Language and Social Psychology, Oxford: Blackwell, 88-120.

Segalowitz, N and E Gatbonton
1977 'Studies of the non-fluent bilingual', in P Hornby (ed.) Bilingualism: Psychological and Social Implications, New York: Academic Press.

Sherif, C W, M Sherif and R E Nebergall
1965 Attitude and Attitude Change, Philadelphia: Saunder.

Shuy, R
1977 'Problems of communication in the cross-cultural medical interview', ITL: A Review of Applied Linguistics 35.

Siebold, D P
1975 'A formalization of attribution theory: Critique and implications for communication'. Paper presented at the Annual Conference of the Central States Speech Association, Kansas City, April.

Siegman, A and S Feldstein
1978 Non-verbal Behaviour and Communications, Hillside, NJ: Lawrence Erlbaum Association.

Simard, L, D M Taylor and H Giles
1976 'Attribution processes and interpersonal accommodation in a bilingual setting', Language and Speech 19.

Smith, P M, H Giles and M Hewstone
1980 'Sociolinguistics: A social psychological perspective', in R St Clair and H Giles (ed.) Social and Psychological Contexts of Language, Hillside, NJ: Colbaum, 283-98.

Summerfield, A B
1975 'Errors in decoding tone of voice during dyadic interaction', British Journal of Social and Clinical Psychology 14: 11-18.

Sunshine, N J and M K Horowitz
1968 'Differences in egocentricity between spoken and written expression under stress and non-stress conditions', Language and Speech 11: 160-66.

Tajfel, H
1974 'Social identity and intergroup behaviour', Social Science Information 13: 65-93.

Tajfel, H and J Turner
1979 'An integrative theory of intergroup conflict', in W G Austin and S Worchel (ed.) The Social Psychology of Intergroup Relations, Monterey, Ca: Brooks/Cole, 33-47.

Tajfel, H and A L Wilkes
1963 'Classification and quantitive judgement', British Journal of Psychology 54: 101-14.

Taylor, D A and I Altman
1975 'Self-disclosure as a function of reward-cost-outcomes', Sociometry 38: 18-31.

Taylor, D M, J Bassili and F E Aboud
1973 'Dimension of ethnic identity: An example from Quebec', Journal of Social Psychology 89: 185-92.

Taylor, D M and V Jaggi
1974 'Ethnocentrism and causal attribution in a South Indian context', Journal of Cross-Cultural Psychology 5: 162-71.

Taylor, D M, R Maynard and E Rheault
1977 'Threat to ethnic identity and second language learning', in H Giles (ed.) Language, Ethnicity and Intergroup Relations (European Monographs in Social Psychology), London and New York: Academic Press, 99-118.

Taylor, L L, J C Catford, A Z Guiora and H L Lane
1969 'Psychological variables and the ability to pronounce a second language', Studies on Language and Language Behaviour 8: 22-38.

Thibaut, J W and H W Riecken
1955 'Some determinants and consequences of the perception of social causality', Journal of Personality 24: 113-33.

Triandis, H C
1960 'Cognitive similarity and communication in a dyad', Human Relations 13: 175-83.

Trudgill, P
1974 Sociolinguistics, Harmondsworth: Penguin.

Turner, J
1975 'Social comparison and social identity: Some prospects for intergroup behaviour', European Journal of Social Psychology 5: 5-34.

Turner, J and R J Brown
1978 'Social status, cognitive alternatives and intergroup behaviour', in H Tajfel (ed.) Social Identity and Social Comparison: Studies in Intergroup Behaviour (European Monographs in Social Psychology), London and New York: Academic Press, 201-34.

Walster, E, E Aronson and D Abrahams
1966 'On increasing the persuasiveness of a low prestige communicator', Journal of Experimental Social Psychology 2: 325-42.

Watzlawick, P, J H Beavin and D D Jackson
1967 Pragmatics of Human Communication, New York: Norton.

Webb, J T
1970 'Interview synchrony: An investigation of two speech rate measures in an automated standardized interview', in A W Siegman and B Pope (ed.) Studies in Dyadic Communication, Oxford: Pergamon.

Welkowitz, J, G Cariffe and S Feldstein
1976 'Conversational congruence as a criterion of socialization in children', Child Development 47: 269-72.

Welkowitz, J and S Feldstein
1969 'Dyadic interaction and induced differences in perceived similarity', Proceedings of 77th Annual Convention of American Psychological Association 4: 343-44.

1970 'Relation of experimentally manipulated interpersonal perception and psychological differentation to the temporal patterning of conversation', Proceedings of the 78th Annual Convention of the American Psychological Association 5: 387-88.

Welkowitz, J and M Kuc
1973 'Interrelationships among warmth, genuineness, empathy, and temporal speech patterns in interpersonal interaction', Journal of Consulting and Clinical Psychology 41: 472-73.

Welkowitz, J, S Rothstein and S Feldstein
1973 'Temporal speech patterns as indices of interpersonal perception'. Paper presented at the Society for Psychotherapy Research, Philadelphia, June.

Wish, M and S J Kaplan
1977 'Towards an implicit theory of interpersonal communication', Sociometry 44: 234-46.

Witkin, H A, R B Dyk, H F Faterson, D R Goodenough and S A Karp
1962 Psychological Differentiation, New York: Wiley.

Wober, M
1967 'Towards a theory linking ability, personality and culture in education', Journal of Special Education 1: 347-56.

Wolff, H
1959 'Intelligibility and inter-ethnic attitudes', Anthropological Linguistics 1: 34-41.

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LANGUAGE, SOCIAL KNOWLEDGE AND INTERPERSONAL RELATIONS*

It has long been recognized that language use in all communities is diverse. Recent sociolinguistic research has demonstrated that diversity characterizes all levels of linguistic structure, and that it relates directly to social differences. The usual method of demonstrating the relationship of linguistic to social phenomena has been to focus on groups previously delimited by non-linguistic criteria such as residence, class or ethnic background and to isolate phonological, syntactic or semantic indices which show a systematic relationship to the macro-sociological variables that partition such groups. Sociolinguists have argued that to study the social functioning of language, the traditional methods of linguistic analysis which rely on long term in-depth hypothesis testing between investigator and key individual informants should be abandoned in favour of survey techniques where large numbers of informants reply to predetermined lines of questioning. The claim is that only statistical abstraction from a broad sample enables the investigator to transcend variation due to personality traits and momentary performance characteristics.

Survey approaches are very successful in characterizing language usage patterns of large populations and have made key contributions to the study of processes of change and diffusion, but they do not deal with the problem of social meaning, that is, the symbolic significance of alternate linguistic choices. We rely on shared features of language both to assess what speakers are *doing* through talk and at the same time to make judgements about them. The basic question of how such micro-social evaluations relate to the macro-social characteristics of speech gleaned from statistical abstraction has as yet been unresolved.

Furthermore, the correlation of linguistic variables with social groups has begun from the assumption that social groups are identifiable and known. This, however, is an issue much in dispute in the social sciences. That is, the question of what a social group is has been very problematic, especially in urban areas, where much sociolinguistic work has focussed. Secondly, the elicitation of valid information from large numbers of speakers is a vast problem, especially since it is crucial that responses truly reflect their habitual performance and knowledge. The more closely we study pragmatic meaning, the more disagreement arises about interpretation and appropriateness.

We would like to suggest a way of approaching the problems outlined which (1) avoids *a priori* identification of social groups, but rather builds on empirical evidence of conversational cooperation, and (2) extends the traditional linguistic method of in-depth hypothesis testing with key informants, to the process of conversational inference.

Recent theoretical studies in philosophy of language and ethnography of communication suggest a theoretical basis for new empirical approaches to social meaning. In contrast to earlier semantic analyses which were concerned almost entirely with lexicon and with the relation of words to things, philosophers of language, following Wittgenstein and Austin, now concentrate on speaking as an activity and on the effect that utterances produce (Grice,1971; Searle,1971). A basic distinction is made among (a) the propositional content of an utterance, that is, the lexical information it carries; (b) the illocutionary force, that is, what is reveals about what the speaker intends to achieve with his utterance; and (c) the perlocutionary effect, the impact that a message has on the listener. While ultimate effects are never entirely predictable, both propositional content and illocutionary force are communicated, and interpretation processes are subject to semantic analysis.

Examination of a number of common utterance types reveals some striking differences between propositional content and illocutionary force. For example, sentences with the syntactic form of questions, such as *May I have a match, Can you open the door* are generally interpreted as requests. *What's happening* is a common greeting. Statements like *Your car is blocking my driveway* may count as directives. In order to account for such discrepancies, it has been suggested that the hearer's interpretation of what a speaker means in any one utterance is the result of a process of inference in which the propositional content of messages is reinterpreted in the light of certain context-bound presuppositions (Searle,1971). To some extent, such presuppositions are a function of physical settings and of such factors as participants' personal knowledge about each other's background, the history of previous transactions and of topics discussed. Presuppositions, however, also depend on social expectations concerning the nature of the verbal activity enacted (for example, conversation, discussion, argument, formal negotiation, verbal game) and concerning the social relationship or the mutual rights and duties among participants that this activity implies. If a speaker, as part of a conversation, says *This room is quite cold*, the hearer who feels obliged to make the speaker comfortable may interpret the remark as a request to close the window or turn up the heat. If the same utterance is said as part of an argument, or in the course of joking or banter, the same interpretation does not follow.

Although a great deal of work is now being done on the linguistic analysis of processes that underlie the interpretation of such indirect speech acts, and it is generally agreed that context is crucial to understanding, analysis, for the most part, deals with single sentences or two or three sentence sequences. What is meant by context and how context enters into the interpretation process has not been systematically discussed. It has, furthermore, been assumed that contextual assumptions are shared and that their interpretation presents no problem.

Work with everyday verbal interaction sequences, such as those illustrated in our examples, indicates that these assumptions are not justified. Setting, discourse topic, and background knowledge

of participants constrain the type of verbal activity that can be enacted, but *they do not uniquely determine it.* No set of extra linguistic social rules can account for subtle changes in mood, mode, and quality of talk, as have been described in Erving Goffman's recent Frame Analysis (1975). To account for the shifts in focus within any one setting, we must assume that judgements of what activity is enacted are *semantic judgements* which are signalled, in large part, through verbal cues. We hypothesize that, at the start of any one verbal encounter, a speaker, building on his background knowledge, makes a semantic judgement about what activities can normally be enacted. Once talk begins, this judgement is then either confirmed and sharpened or altered by assessing discourse topic, non-verbal cues, as well as contextualization cues of the type discussed in connection with our examples. At the verbal level, then, a speaker's co-occurrence judgements about the relationships between topic and contextualization cues play an important part in signalling the contextual and social presuppositions in terms of which component messages are to be interpreted.

The following example recorded during a helicopter trip will illustrate the point. As the craft begins to descend, the stewardess picks up the microphone and says:

(5) [1] S: We have now landed at San Francisco where the local time is 10:15. We would like to thank you for flying SFO Airlines and wish you a pleasant trip. [The stewardess then continues over the PA system, with only a slight pause] Isn't it quiet around here? Not a thing moving.

The first passage is spoken in staccato rhythm, even stress and contoured intonation. Both topic and prosody identify it as an announcement. In the next passage, rhythm, intonation and stress shift to suggest conversation. The shift caused the audience to look around and provoked comments like *I wonder what's happening.* Others replied by saying that some ground personnel were on strike. Note that the distinction between announcing and conversing represents more than a difference in label; there is also a shift in illocutionary force. The announcement has the force of a representative (Searle,1974), and the intent is to conform to a legal requirement. If a passenger had responded to the stewardess *We thank you...* with *You're welcome,* his remark would have been interpreted as impertinent or, perhaps, a bad joke. The second passage, also a representative from the point of view of speech act theory, affects the hearer as a conversational statement intended to provoke a verbal response, which it does. Its force is clearly different.

A similar shift in verbal activity is signalled in the last sentence of example 2. This sentence is spoken with unusually slow rhythm. Throughout the tape, this same rhythm is used in reading and is maintained even when the student knows and can easily decipher the words in question.

Apart from their role in signalling verbal activities, contextualization cues have other semantic functions. They can also topicalize a phrase within a sentence that forms part of what is otherwise seen as a unitary verbal exchange. This can be seen in the following example from a class in language arts for low-income, inner-city, sixth-grade black children taught by a black college student:

(1) Student: [reading from an autobiographical essay] This lady, didn't have no sense.
Teacher: Do you know another way of saying that?
Student: Sure. She didn't have any sense. *But not this lady; she didn't have no sense.*

The dialect phrase *she ain't got no sense* is embedded in a standard English passage. Here, the use of forms ordinarily associated with in-group black activities metaphorically suggests that the word *sense* is to be interpreted in its black meaning (see Blom and Gumperz, 1972 for a more detailed discussion of such metaphoric usages).

As our discussion suggests, contextualization cues can take a number of linguistic forms depending on the linguistic repertoire (Gumperz,1971) of the participants. Apart from shifts in stress, intonation, and rhythm, choice of pronunciation, choice of words or syntactic structure, choice of dialect or language in bilingual situations, the use of stereotyped openers or sequencing strategies (Schegloff,1972) may all have similar contextualization functions. Although all such cues can carry meaning, whatever meanings are conveyed, are conveyed in the course of the speech activity. Unlike words that can be discussed out of context, the meanings of contextualization cues are implicit and cannot be talked about directly. Their signalling value depends on the participants' tacit awareness of their meaningfulness. When all participants understand and notice the relevant cues, there is no problem. However, when a listener does not notice a cue or is unaware of its function, interpretations may differ and misunderstanding may occur. Note that when this happens and when a difference in interpretation is brought to participants' attention, it is talked about in attitudinal terms. A speaker is said to be unfriendly, impertinent, rude, uncooperative, or failing to understand - it is not ordinarily noticed that he may have failed to perceive a shift in rhythm or a change in pronunciation. Miscommunication of this type, in other words, is seen as a social *faux pas* and leads to misjudgements of the speaker's intent; it is not likely to be identified as a mere linguistic error.

The cues involved here are gradual or scalar; they do not take the form of discrete sequents. What is involved is a change from normal in one or another direction. But while semantic directionality is, in large part, universal, the situated interpretation of the meaning of any one such shift in context is always a matter of social convention. There are conventional notions about what count as normal and what count as marked kinds of rhythm, intonation and loudness. By signalling a speech activity, a speaker also signals the social presuppositions in terms of which his/her message is to be interpreted. Conventionalized notions of normality also differ within what, on other grounds, counts as a single speech

community. When this is the case, as when the teacher fails to appreciate the semantic import of children's systematic use of intonational and rhythmic cues, miscommunication can occur.

Furthermore, there are other contextualization cues which have completely conventionalized meanings. An example of this comes from an oral report by a graduate student in educational psychology, who served as an interviewer in a survey. He was sent to interview a black housewife in a low-income neighbourhood. The contact had been made over the phone by someone in the office. The student arrives, rings the bell, and is met by the husband, who opens the door, smiles, and steps toward him saying:

(4) Husband: So y're gonna check out ma ol lady, hah?
Interviewer: Ah, no. I only came to get some information. They called from the office.
[Husband, dropping his smile, disappears without a word and calls his wife.]

The student reports that the interview that followed was stiff and quite unsatisfactory. Being black himself, he knew that he had 'blown it' by failing to recognize the significance of the husband's speech style in this particular case. The style is that of a verbal game, used to 'check out' strangers, to see whether of not they can respond with the appropriate idiom. Intent on doing well in what he saw as a formal interview, the interviewer failed to notice the husband's stylistic cues. Reflecting on the incident, he himself states that, to show that he was onto the husband's game, he should have replied with a black dialect phrase like *Yea, I'ma git some info* ('I'm going to get some information') to prove his familiarity with local verbal etiquette and culture. Instead, his standard English reply was taken by the husband as an indication that the interviewer was not one of them and, perhaps, not to be trusted.

The husband's opener *So y're gonna check out ma ol lady*, when said with the right stress and intonation, is one of a set of idiomatic openers for verbal game used in some black groups. The communicative uses of code switching are similarly conventionalized and rely on speakers' knowledge of the values associated with the codes in question, as are the often highly complex linguistic constraints that govern the use of code switching strategies (Gumperz,1975). Since the conventions governing the interpretation of contextualization cues are not overtly verbalized, they must be learned indirectly through regular and direct association with their users. Understanding of contextualization cues is, therefore, in large part, a matter of shared background, of similar past communicative experience and values; where communicative backgrounds differ, as in modern ethnically diverse urbanized societies, these differences will also be associated with difference in contextualization cues. Because these cues function indirectly, empirical analysis is a major problem in the study of intracultural communication.

New types of discovery methods are needed to identify differences in the perception of cues. The method we have begun to work out relies on the isolation of illustrative passages from natural

conversation just long enough to form a proper context. These are both transcribed, literally, and described to form a written and tape-recorded record which can then serve as a basis for eliciting judgements from speakers about what cues they respond to in arriving at particular interpretations.

We have isolated a number of typical instances of interpretation differences and have experimented with eliciting strategies capable of making explicit the unverbalized perceptions and presuppositions that underlie speakers' interpretations. Here are some examples:

(6) Conversation between husband (A) (middle-class American) and wife (B) (middle-class British).

	Transcription	Description
A:	Do you know where today's paper is?	A, using question form, asks for location of newspaper.
B:	I'll get it for you.	B does not reply to question but offers to get the paper. *I'll* is marked by stress and slightly raised pitch. Suggesting annoyance?
A:	That's OK. Just tell me where it is. I'll get it.	A retort that he is asking for information, not making a request. Stress on *I'll*.
B:	No, I'LL get it.	B reiterates offer, with *I'LL* now marked by very emphatic stress, suggesting increasing annoyance.

(7) Conversation between mother (A) and 11-year-old son (B).

	Transcription	Description
A:	Where are your boots?	A uses unmarked question form to ask for location of shoes.
B:	In the closet.	B replies in unmarked statement form giving the location.
A:	I want you to put them on RIGHT NOW.	A retorts with a request. The *RIGHT NOW*, said with stress and raised pitch, suggest annoyance and imply that B should have put on his boots after the first question.

(8) Conversation between an undergraduate employed as a research assistant (A), who is busy writing at his desk, and a faculty member, his supervisor (B), who is passing by at some distance. The two are on first-name terms.

	Transcription	Description (partial)
A:	Hey, John, come and help me with this. I'm putting it all down.	A calls out, requesting B to step over and assist him.
B:	What is it?	B retorts, asking the reason for the request.

A:	I'm almost done. I just need to fix it up a little.	A markedly fails to identify what is wanted and reports on the details of what is being done.
B:	What do you want me to do?	
A:	I'm writing down everything just the way you said.	
B:	I don't have the time right now.	

(9) Telephone conversation between a faculty member (A) and a student research assistant (B).

	Transcription	Description
	The telephone rings.	
A:	Hello.	A responds to ring with unmarked conventional response.
B:	How's the family?	B retorts with a conventional question about A's family. The retort is a marked absence of a greeting (eg Hi, John) or identification (This is X).
A:	Fine.	A retorts with conventional answer, implying that he recognized B's voice.
B:	I'll get back to you next month about that thing.	
A:	That's OK. I can wait.	
B:	I'm finished with that paper. It's being typed.	
A:	Come to the office and we'll talk about it.	

Taken by themselves these passages reflect quite ordinary everyday exchanges. Similar incidents have long been analysed by small group researchers, students of psychiatric case records, sociologists, and others. Component passages have been examined for what they reveal about participants' psychological characteristics and their role relationships with others. In making such analyses, however, analysts tend to assume that evaluation of the verbal evidence itself presents no problem, that it is simply a matter of testing for conformity to generally accepted rules of etiquette and enunciation.

In order to show that such assumptions are unjustified, these and similar recorded examples were played to listeners of different communicative and ethnic backgrounds. Elicitation strategies such as the following were used to determine differences in marking and contextualization cues:

1. The incident was first played in its entirety, then played again sentence by sentence.

2. The respondent was asked a series of probing questions, each of which builds on the preceding response:

   a. What is A trying to achieve by talking in that way? What impression does he make?

   b. What is it about the way he says it that makes you think he is trying to...?

   c. Could he also be trying to...?

   d. How should he have said it if he wanted to...?

   e. How did B interpret what A said?

   f. How can you tell that B misunderstood?

   g. How should B have replied to show that he did understand?

The initial questioning concentrates on subjective evaluations of content and quality. Subsequent questions attempt to induce judges to relate their evaluations to the surface form of what is said. We are looking for comments such as the speaker 'raised his/her voice', 'his/her tone dropped', 'is being too loud', 'used...'. Given such comments, the passages in question can then be given detailed transcriptions to identify the relevant phonetic or prosodic cues.

Whenever a judge identifies some special expression or mode of talking, a range of alternative expressions, which lead to different interpretations, is also elicited. All answers are evaluated for their relevance to the present context. If necessary, the passage in question is played over several times. Furthermore, elicitation concentrates on exchanges consisting of speaker A's remarks and B's responses, not on a single utterance. Our main goal is to investigate the relationships between listeners' interpretations and identifiable features of message form and content, *not to get at 'true' or 'real' meanings*.

Examination of examples 6-9 in these terms reveals a number of differences in interpretation. Some judges identify the first utterance in example 6 as a request for information, often as a request: others again suggest that it is ambiguous. Some interpret the mother's remark in example 7 as an order to put on boots: others feel it could be a request. In example 8, a number of judges note A's failure to state clearly what he is doing and what he wants B to help him with. These same judges also note B's failure to say hello in the telephone conversation of example 9 and suggest that this omission seems rude. Others, however, instead of mentioning A's vagueness in example 8, claim that B's insistence on asking what is wanted is out of place. A common comment was: 'Why didn't B say that he doesn't have any time in the first place?'

At first glance, these evaluations seem to reflect individual interpretations of what are essentially inherent ambiguities or differences in degree. Although some trends begin to emerge, it

would be premature to claim that they relate to cultural background. But when we examine choice of alternative expressions and sequencing strategies, more systematic relationships begin to emerge.

Judges who identify the husband's opener in example 6 as a request suggest that the wife's annoyance is justified since, if he did not want her to get the paper, he would not have used that expression. He would have said, *I wonder where the paper is*. These same judges also claim that the child's answer to the mother's order in example 7 is impertinent, and that this justified the mother's annoyance. What the child should have done to justify himself, they say, is to answer indirectness with indirectness and reply with something like *Why, is it raining?*

Those individuals who identify the openers in examples 6 and 7 recognize their use as meaningful strategies. Patterns become clearer with example 9, where everyone notes the caller's failure (a) to identify himself and (b) to open with a greeting such as 'Hi'. Since the participants know each other and can be presumed to recognize each other's voices, self-identification is not necessary, but the lack of a greeting is significant. Some judges merely state that this seems odd, or perhaps rude. Others, however, recognize it as part of a meaningful strategy, an indirect way of suggesting that the speaker wants something. Frequently, they illustrate their comments with anecdotes from their own experience, citing similar verbal strategies. These same judges point to the speaker's failure to state what he wants in example 8 as a similar instance of indirectness. The strategy underlying both examples seems to be something like this: Do not verbalize explicitly what the conversation is about, rely on the listener's background knowledge. If he is a friend, he will guess what is wanted and will cooperate, and, if he enters into this type of interaction at all, he can be presumed to understand.

The mark of failure to say something that is normally expected can be seen as a conventionalized contextualization cue. It is meaningful in the same sense that the use of the idiomatic opener in example 4 and the switch to black English in example 1 are meaningful. Whenever a particular subgroup of listeners (a) identifies such conventionalized meaningful contextualization cues and (b) agrees on their meaning and on appropriate sequencing strategies, while other subgroups do not see these cues as meaningful, we have fairly good evidence that the difference in interpretation also reflects significant differences in communicative background. It can be shown that the use of such conventionalized cues, being both covert and highly context bound, is not always easy to learn. They are learned primarily in the course of informal peer group interaction under conditions allowing for maximum feedback, and this suggests some commonality of family or ethnic background (for more detail, see Gumperz and Gumperz,1976). In fact, judges who see meaningful indirectness in examples 8 and 9 are either black or are familiar with black rhetoric. We might tentatively identify the relevant strategies as reflecting black rhetorical style.

The interpretations of examples 6 and 7 represent differences in degree or quality, rather than in conventionalization. The signalling cues here bear similarity to degrees of pitch height or loudness. We might visualize the distinction in terms of the following scale involving different degrees of requestness:

1. Get me the paper.
2. Can you get me the paper?
3. Do you know where the paper is?
4. I wonder where the paper is.

Here the semantic distinction between requests and questions takes the form of what Ross (1974) calls a 'squish'. Subgroups of speakers differ on where to draw the line between a question and a request. The evidence of relation to differences in cultural background is somewhat weaker than in the case of conventionalization, but it does seem significant that the mother and wife in examples 6 and 7 are English and that other English judges tend to favour the request interpretation, while Americans tend toward the question interpretation.

More data is needed, based on larger and more varied samples of interaction. It should be pointed out, however, that, by relating perception and interpretation of contextualization cues to cultural background, we are not attempting to predict usage or to relate the incidence of linguistic variables to other characteristics. The goal is to identify strategies of interpretation that are potentially available to speakers of certain backgrounds, and to alert people to the ways in which culture can affect interpretation of seemingly unambiguous messages.

Once such strategies are identified, it becomes possible to test our understanding of their meaning and distribution by constructing more systematic tests to be used with larger samples of judges. These tests take the form of alternate verbalizations of similar socially realistic episodes, built on naturally occurring examples which are recorded by good mimics who are familiar with the relevant strategies. These can be submitted to ethnically and occupationally stratified samples, and results are subject to statistical analysis of the usual kind.

The following key sentence, extracted from a test that we are constructing to deal with communication problems of students from India who are used to Indian styles of English, may serve as an example:

A: You may run all the way to the Post Office, but I'm sure it will be closed by the time you get there.

Question: Which of the following two statements is closest to what the speaker really meant?

a. It doesn't matter whether or not I give you permission to go to the Post Office. Even if I do and if you run, you won't make it before closing time.

b. It is possible that you could run to the Post Office, but it will be closed by the time you get there.

The linguistic issue here hinges on the meaning of the modal *may*. Speakers of American English use *may* to mean either 'permission' or 'possibility'. Speakers of Indian English in India use it only to indicate permission. Our results with this type of question show that recently arrived speakers of Indian English unanimously choose interpretation (a); speakers of American English choose interpretation (b). Indian students who have lived in the United States for some time will be aware of interpretation (b) if they have lived in typically American settings and have formed close friendships with Americans. Those who have lived surrounded by other Indian friends are less likely to be aware of interpretation (b). Understanding of communicative strategies is, thus, less a matter of length of residence than of communicative experience.

Misunderstandings caused by such interpretation differences reflect phenomena that are typically sociolinguistic, in the sense that their interpretative weight is much greater than their purely linguistic import. Whenever they occur, they have the effect of retrospectively changing the character of what has gone before, of reshaping the entire course of an interaction (Garfinkel, 1967; Sacks, 1972). A mistake in one such feature would lead the listener to think, 'I thought we were on the same wave length, but we are obviously not'. Although once cues are discovered, their linguistic nature is quite apparent, discrepant practices may persist despite years of intergroup contact. Often, speakers may be aware of vague difficulties in communication or of their inability to involve others in serious talk, yet rarely do they see, nor does conventional grammatical analysis suggest, that such difficulties may have linguistic causes.

CONCLUSION

Thus we have shown that linguistic features, or contextualization cues, are the means by which (1) shared social knowledge is used to signal how utterances are to be interpreted and (2) the fact that this social knowledge is shared is signalled. Focus on misunderstandings makes it possible to isolate processes of contextualization which go unnoticed when communication is effective, for these are aspects of language which people rarely notice and almost never talk about.

Our method bases analyses on actual conversations. Interpretations are elicited from participants themselves as well as other informants, thus combining linguistic techniques of intensive questioning with sociological techniques of broad sampling. Identification of social groups is then the result of empirically-deduced patterns of interpretation of language. In this way, the structure of language is seen as the concrete evidence as well as one of the determiners of social structure.

FOOTNOTES

* The data on which this paper is based were presented in an earlier paper (Gumperz, 1976). I am grateful to Jenny C Gumperz for her help in clarifying many of the basic concepts. Maurice Bloch, Claudia Mitchell Kernan, Eugene Hammel, and Erving Goffman made helpful comments on our earlier draft. Deborah Tannen assisted in the preparation of this draft.

1. In numbering example sentences, the same numbers have been retained as used by the author for the same examples in other papers of his.

REFERENCES

Blom, J P and J J Gumperz
1972 'Social meaning in linguistic structures: Code-switching in Norway', in J J Gumperz and D Hymes (ed.) Directions in Sociolinguistics, New York: Holt.

Garfinkel, H
1967 Studies in Ethnomethodology, Englewood Cliffs, N J: Prentice-Hall.

Goffman, E
1975 Frame Analysis, New York: Harper and Row.

Grice, H P
1971 'Utterers' meaning, sentence meaning and word meaning', in J R Searle (ed.) The Philosophy of Language, Oxford: U.P.

Gumperz, J J
1971 Language in Social Groups, Stanford: U.P.

1975 'Code-switching in conversation'. Unpublished manuscript.

1976 'Communication and public negotiation', in P Sanday (ed.) Anthropology and the Public Interest, New York: Academic Press.

Gumperz, J J and J C Gumperz
1976 Language, Speech and Social Action, New York: Academic Press (in preparation).

Ross, J R
1974 'A fake NP squish', in C-J Bailey (ed.) New Ways of Analyzing Variation in English, Washington DC: Georgetown U.P.

Sacks, H
1972 'On some puns with some insinuations', in R Shuy (ed.) Sociolinguistics: Current Trends and Prospects, Washington DC: Georgetown U.P.

Schegloff, E
1972 'Sequencing in conversational openings', in J Gumperz and D Hymes (ed.) Directions in Sociolinguistics, New York: Holt.

Searle, J R
1971 'What is a speech act', in J R Searle (ed.) The Philosophy of Language, Oxford: U.P.

Searle, J R
1974 'A taxonomy of illocutionary acts', in K Gunderson (ed.) Minnesota Studies in the Philosophy of Language, Minneapolis: U.P.

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COMPARING LANGUAGES IN CONTACT

The study of langauge contact is not the exclusive province of linguistics. It has also been the concern of psychology, sociology, education, law, anthropology and other human sciences. Some of the basic issues have been discussed by groups of specialists from different fields in an attempt to obtain a multidisciplinary view of the problem (Kelly, 1969). Taken together these various disciplines have produced a vast literature on the many questions of language contact. The first volume of the International Bibliography on Bilingualism, for example - which is far from complete - lists more than eleven thousand titles, whereas the second volume now in preparation already contains some additional eight thousand (Mackey, 1972).

The linguistic study of language contact has itself developed into a rich field of inquiry which has been skilfully surveyed on two occasions by Einar Haugen (Haugen, 1973 and 1956). It is not the intention of this chapter to duplicate these works which, although they concentrate on the Americas, are of universal validity in the principles, problems and methods they explain.

In the present context, this chapter will have to be limited to a treatment of the methods of language comparison as practised by students of language contact in an attempt to show how and why these have differed from the techniques of contrastive linguistics.

After searching the literature on language contact under the terms 'contrast' and 'constrastive' we found very little indeed in the way of reference to these terms. The reason for this is not hard to find. Whereas contrastive linguistics, as Bolinger has pointed out, was born in the classroom (Di Pietro, 1971), language contact studies have been part of a long tradition of comparative linguistics which goes back to the early nineteenth century, as any good history of linguistics will attest. The names in the following paragraph and the titles of works associated with them can readily be found in introductory manuals of linguistics.

An elementary chapter on the history of linguistics will recount how the comparative method was first confined to the historical study of genetically related languages (Grimm, 1822). For more than a century this method was perfected by succeeding generations of linguists such as Bopp (1833), Diez (1836), Schleicher (1861), Schmidt (1872), Whitney (1874), Paul (1880), Brugmann and Delbrück (1886), Pokorny (1919), Jespersen (1922), Vendryès (1925), Meillet (1928) - to name only these. The comparative method was to adopt and encompass the preoccupations of general and descriptive linguistics as propounded by von Humboldt (1836), Steinthal (1861),

Wundt (1900), Müller (1888), Saussure (1900), Trubetzkoy (1915), Sapir (1921) and Bloomfield (1935) to develop into the most exact discipline of all the human sciences. It had become evident to the nineteenth century comparatists that the only significant historical study of a language had to be based on comparison of two or more sets of descriptive data. And it had even become axiomatic that sound descriptive study is the basis of all historical, comparative and psychological generalizations about language.

All comparatists did not, however, limit their inquiry to historically related languages. Müller's brief structural sketches of 1876 were not based on historical relationships. In 1910 Finck did a series of descriptions of unrelated languages. Common features of historically unrelated languages were also studied by Jespersen (1922), Meillet (1924), Vendryès (1925) and others. It was pointed out that genetically unrelated languages occupying the same geographical area often possessed certain structural affinities. This *Sprachbund* was attributed to the transfer of features over the centuries as a result of language contact (Sandfeld, 1930). But what features were actually transferred seemed to depend on the structures of the languages in contact (Mackey, 1953).

Many of the features which cropped up in the evolution of a language could not always be explained by the laws of evolution or analogy. One had to postulate interference from an outside source. In 1872, Schuchardt's work on the evolution of Albanian led him to a long study of the influence which one language could have upon another in the form of language mixture and creolization. The preoccupation with language contact became more and more prominent in the context of comparative linguistics (Schuchardt, 1872); Grünbaum (1885) identified the presence of mixed languages. It had become increasingly evident that not all changes in the evolution of languages could be attributed to analogy or to the phonetic law. It was suggested that the spread of a language over a wide area and its rapid adoption by adults speaking another language would result in a transfer of features from one language to the other - especially when the new speakers became the dominant majority. This was presumably the case with the spread of Latin in many parts of the Roman Empire, resulting in the birth of the widely divergent Romance languages. Such an explanation of language divergence as a result of language contact became known as the substratum theory. Although this theory was extended by some to suggest permanent racial influences upon an adopted language, evidence was quickly forthcoming to discredit this mythical influence. It was also demonstrated that changes could be brought about in the language of the area as a result of conquest and dominance by peoples speaking another language, as was the case of the adoption of the Scandinavian elements in English. This phenomenon became known as the superstratum. It was pointed out, however, that there could be contact without dominance and that this contact could also result in the transfer of features. The presence of such features in a language were called the adstratum. These theories were based on the assumption that if the appearance of a new feature in the evolution of a language could be attributed neither to the laws of language change nor to the effects

of internal analogy, then the cause must be sought outside the language itself in such forces as the influence exercised by another language (Rozencvejg, 1972: 51-78).

If such a feature could be located in a language with which there had been contact then the change was attributed to language contact. This *post hoc ergo propter hoc* type of reasoning was never quite satisfactory, since it did not conform to the rigour which the science of comparative linguistics had developed in the study of the internal evolution of linguistic forms.

It was not until the 1930's that more exact methods developed in establishing proof of transfer from one language to another. The desire to establish such methods was reflected in the preoccupations of the periodical International Congress of Linguists which had been established in the late 1920's. In the Fourth Congress the study of 'language affinities' was one of the topics and it attracted papers from such linguists as Meillet, Jakobson and Sandfeld. And language contact and the influence of one language upon another was one of the main questions on the agenda of the following but aborted Fifth International Congress of Linguists scheduled for the autumn of 1939 in Belgium at a time when the armies of Nazi Germany were already beginning their invasions in Europe. At every Congress since then there has been some concern for problems of language contact: at the Sixth in 1948 (Vogt), the Seventh in 1952 (incidentally), the Eighth in 1957 (Haugen and Weinreich), the Ninth in 1962 (Badia Magarit, Galinsky, Lanhams and Philipp) and more than a dozen papers at the Tenth Congress in 1967.

It was only after the war that rigorous methods of proof of language transfer were developed. These were based on exact synchronic bilingual descriptions of the dialects of the particular language which were, or which had been, in contact. These descriptions were done within the scientific tradition of comparative linguistics.

When the word contrastive was applied to the comparison of languages in the 1940's, it was not surprising that students of language contact did not readily accept the term, for they knew that it was the similarities as much as the contrasts which accounted for interference in the speech of bilinguals, and they saw no reason why they should adopt a new terminology allegedly invented for the purpose of interesting language teachers in the study of the products of a certain school of descriptive linguistics. In the 1950's Einar Haugen, the leading American authority on language contact, summed up the situation as follows: 'The term *contrastive linguistics* has been proposed (by Trager), but the word contrast has another meaning also, and beside this, seems to exclude the comparison of like items in two languages' (Haugen, 1956:41). Almost twenty years of 'contrastive' studies did not change Haugen's opinion as far as the study of language contact was concerned. He found the term *contrastive analysis* 'even more confining' than other terms (Haugen, 1973: 522). Weinreich had used the term *differential analysis*, a term which had also been familiar to students of translation and the one which later became current in the field

(Darbelnet, 1971). Other terms, of course, had also been proposed, including such expressions as *diglossic grammar* (Dingwall), *transfer grammar* (Harris) and *dialinguistics* (Haugen). Haugen himself has preferred to stick to the simple term *bilingual description*, since 'it was meant to designate a point-by-point comparison of two or more languages in the same way as a bilingual dictionary compares the lexicon of two languages' (Haugen, 1973: 522). In this way, bilingual descriptions could include not only phonology and grammar, but also a comparison of all lexical, stylistic and socio-linguistic elements of the two languages. The whole could be integrated into a single over-all description to produce a dictionary-like product which would be of much greater use to the individual since it would go far beyond the limits of current bilingual dictionaries (Darbelnet, 1970).

Haugen (1973) attributes the popularity of the term 'contrastive' primarily to its acceptance as part of an applied linguistics where it was promoted as a scientific technique to improve the quality of foreign language teaching. Many such contrastive studies were produced before anyone tried to demonstrate whether or not such studies actually did improve the quality of foreign language teaching (Oller and Richards, 1973). Such studies had been undertaken in Europe long before they were re-invented in America (Rheinfelder, 1926). And in the field of language teaching other terms had been used for the activity of comparing the language to be taught with the mother tongue of the learner (Mackey, 1967:Ch 3).

Attempts have been made to link the methods of analysis of language acquisition with those used in language contact studies. In 1971 Filipović suggested a method of *compromise system* analysis (Filipović, 1971). This method could be regarded as an extension of the type of description of *learner systems* proposed by Nemser (1961) to reflect the actual process of language acquisition rather than the static contrasts which presumed to explain the behaviour of learners by means of a mere juxtaposition of linguistic forms. This type of learner system description could in term be considered as an extension of the analysis of *interlingual identification* proposed by Weinreich (1953: 7) and Haugen (1956: 43-50). The process has been found to take place within a single language as well as between two languages (Mackey, 1972).

It has been suggested that all these methods developed by specialists in language teaching, translation theory and the study of language contact be integrated into an autonomous discipline called *interlinguistics* (Wandruszka, 1971). The term has been used to refer to studies on the development of artificial auxiliary languages, as for example, in Martinet's survey for the Sixth International Congress of Linguists; such studies and those related to translation and language contact are not mutually exclusive.

Studies labelled 'contrastive', however, have differed from those called 'bilingual' and 'differential' in features other than name. They have differed in the type of data used, in methods of comparison and in techniques of quantification.

Linguistic data used in language contact studies have usually been those supplied by bilinguals themselves or those found in samples of speech mixture. They have included samples of the speech of bilingual children, the speech of bilingual communities and also that supplied in the study of bilingual literature, bilingual speech pathology and the language introspection of bilinguals themselves.

Techniques of comparison in language contact studies depend on whether one is studying the two codes or analysing the messages produced by their use in the mouths of bilingual speakers. A distinction between the overlapping of two codes and the incorporation of the elements of another code in the speech of bilinguals was made by Weinreich (1953) who, however, used the term *interference* for both phenomena. The distinction was further elaborated by Haugen, who separated the study of *integration* into the code from that of interference in speech. This distinction led to the elaboration of different techniques of analysis.

One of the first developments was the technique for the analysis of integration, a process characterized by the regular use in the code of elements coming from another language system. The study of exactly which features of which language have been incorporated into any given language resulted in the development of exact methods in the analysis of language borrowing. So did the formulation of postulates on which analysis could be based. These included the following: 1) Every speaker attempts to reproduce previously learned patterns. 2) Some of these may belong to a different language. 3) He may produce some in the context of another language (Haugen, 1950). Only if we assume that every speech event belongs to a definite language can we conceive of 'an utterance containing some elements belonging to another language' (Weinreich, 1953).

The first task of the analyst was therefore defined as that of identifying each of the languages in contact. On the one hand there would be the source language - also called the 'lending' or 'donor' language - or, as Haugen has called it - the *model*. On the other hand there would be a recipient, or borrowing, language, called the *replica*. The task of the linguist is to identify the model and the replica (Haugen, 1956: 34). This identification can best be made in the context of a bilingual description the purpose of which is to provide a framework for the study of transfers which do occur from the model to the replica.

The particular study of transfer in both language and speech is based on the analysis of *interlingual identification*, that is, the combination of features of one language with those of another (Weinreich, 1953). This analysis depends on the establishment of *dialinguistic relationships* (Haugen, 1956: 45) to be determined analytically and verified pragmatically. The relationships, which include both form and distribution, may be *diaphonic* or *diamorphic*. A diaphonic relationship may be described in terms of *diaphones* (bilingual allophones or phoneme variants). For example, comparing the nasal phonemes of Spanish (S) and English (E), S/n/ is identified with E [-m,-n, -ŋ]. Haugen expresses such relationships in diaphonic equations of the forms: E/-m,-n,-ŋ/ > n/S.

A diamorphic relationship depends on the phonemic shape of the morphemes and on their meaning. The resulting *diamorph*, may thus be either homophonous or synonymous, or both. For example, the Swedish word for 'heather' *ljung* / juŋ / is homophonous with the English word *young* and is sometimes identified with it by Swedish-English bilinguals (Haugen, 1956: 47). French *plancher* and English *floor* share the same meanings and this synonymy is extended by French-English bilinguals to include the floors of buildings (F *étages*) in expressions like *au cinquième plancher* (on the fifth floor). German *auf* is both homophonous and partly synonymous with English *off*, leading to interlingual identification as reflected in such Canadian-German expressions as *mein Tag auf* 'my day off'.

Both diaphones and diamorphs may be involved in the transfer of an element from the model to the replica (*importation*) or in the substitution of some equivalent of the receiving language (Haugen, 1956: 50).

In addition to methods used for the analysis of two languages in the study of bilingual integration are those used exclusively for the study of bilingual interference, which has sometimes been defined as limited to the study of discourse (Mackey, 1962). Some writers have made a further distinction between interference, code-switching and different types of triggering (Clyne, 1967).

Since samples of discourse can be readily gathered, one method bases the analysis of interference exclusively upon a study of such samples. All levels in the hierarchical structure of utterances are taken sentence by sentence and the elements of each - clauses, phrases, words, morphemes, phonemes and allophones - are allocated by immediate constituent analysis either to one language or to the other - or to both (Mackey, 1965). The method has been integrated into a larger sociolinguistic technique of analysing bilingual behaviour (Mackey, 1966). It has also been compatible to the growing desire to study bilingualism as an over-all pattern of behaviour and human interaction (Gilbert, 1970). The assumption has been that this behaviour includes alternation and a certain admixture of two separate codes which must be described before the degree of admixture can be ascertained. But these codes or local norms may themselves already contain a certain amount of admixture. The problem remains, therefore, of being able to attribute items to a particular language; and in situations of language contact, this may not always be possible.

Since language must evolve, there must be variation and vacillation; otherwise we would always be dealing with dead languages. The speed of language evolution through vacillation varies according to the social elements of control - likely to be different in illiterate and bilingual communities. In bilingual communities, the incidence of interference contributes to the degree of vacillation, and consequently to the speed at which one or more of the languages or dialects evolve. So that degrees of change which in an unilingual situation will take many generations may, under the impact of bilingualism, be realized in one (Mackey, 1970b).

If this is the generation whose language use is being described, the investigator is faced with what appears to him either as interference or as a code with a high degree of free-variation. Both are illusions conditioned by the postulate that we are dealing with synchronic codes. And the treatment of bilingual material as synchronic becomes more and more complex as one multiplies cases, because in any evolving code the degree of individual variation is a function of the rate of change.

And it is precisely the speed of this evolving code in situations of language contact that makes its description and measurement so difficult. What has to be described and measured is a two-dimensional continuum, one of which is continually alternating at the moment the other is inconsistently vacillating.

This fiction of the synchronic, which has served so well in generating the abstractions of descriptive and transformational grammars, becomes quite unreal when used to describe the unstable and rapidly evolving systems of nonliterate communities and of languages in contact. This fallacy which assumes a fixed code or norm has led the students of language contact up a blind alley at the end of which was the impossibility of distinguishing between the two fundamental notions in the linguistic study of bilingualism, namely between integration and interference. This fiction of synchronic description has also made it difficult to determine when interference in the code is no longer interference, that is, when it becomes part of the language (Mackey, 1970b). One may argue that the greater the interference the more likely it is that items be transferred; but this likelihood is a funtion of the recurrence of any given item, not of interference as such.

Repeated interference from another code tends to increase code entropy, whereas literacy and standardization tend to decrease it. Two related unwritten languages will tend to blend into one more quickly than will two equally related languages with standardized written forms used by most of their speakers. And in formally standardized codes, interference will reach the point of resistance earlier than in nonstandardized languages, for the gap between what speakers of standard languages say and what they know they should be saying becomes quickly more apparent and tends to promote in the literate community such formalized defences as purism, irredentism and language repression (Mackey, 1970b).

In the permissible range of variation in usage, the entropy of a language is affected by the degree of tolerance of the people who speak it. Some communities tolerate interference more than do others. This tolerance may have historical determinants; but it is also related to literacy, standardization and language contact. In nonliterate communities it may be wider than in literate societies. In unilingual communities, it may be less than in multilingual groups where the incidence of use of linguistic features from several languages by any individual may actually form a single continuum (Le Page, 1968). And in nonliterate, multilingual groups, the range of tolerance may even become identical to mutual intelligibility achieved by a speech economy whose norm is the sum of all operant

codes. In such situations, the new elements entering the speech of individuals from another language or dialect may do so entirely by chance and never be heard again; or they may be repeated with such consistency as to give the impression that they have been transferred to the other language and integrated into its code. Yet there is no indication in the occurrence of these elements in the stream of speech whether they represent such cases of integration or cases of interference. How then can one distinguish between integration and interference?

One of the most difficult puzzles in the study of bilingualism has been the separation of cases of integration (borrowing) into the code from cases of interference in the message. It involves two problems: (1) the problem of identification, and (2) the problem of relativity. When we listen to an item from another language used in a stretch of speech, we have at first no direct way of knowing whether the item has been integrated into the code of the speaker or whether he is bringing it in from another code. We do not know whether the presence of the foreign item is the result of integration into the code or interference in the message.

Even in highly literate communities there is a permissible range of variation in usage, and on close examination we find that instead of a fixed code and a positive norm, we have an unstable code and a relative norm. The word 'norm', of course, can be ambiguous, meaning either what people expect or what people do. Here we will take the norm to mean what people do and say, not what they say they do. And since all people do not always do the same things in the same ways, the norm is relative. If it were not, languages would be eternal; they would never change (Mackey, 1970b).

New elements are continually entering a language and old elements are dropping out. But this intake and fallout is not sudden; it does not happen the day a new grammar or dictionary comes off the press to consecrate its contents as the norm. It is a gradual process which is observable but not observed. It takes place in time at a rate which is highly variable. And the variability of the rate is a function of numerous factors - some stable and others unstable. But the rate of variation may not be the same for each of the two languages in contact. The two languages which must be described and compared, therefore, are not fixed codes. They are rather systems including many items not all codified to the same extent. In order to describe and compare the languages we need to find out the extent to which these items form part of one code or the other.

By what criteria can we measure the extent to which a foreign item has become part of a language code? We can take our measures either (1) from the message, or (2) from the code. By collecting samples of the speech of bilinguals, it is possible to identify and quantify the foreign elements that are introduced. This can be done from the point of view of their frequency or from the point of view of the range of occurrence.

From the point of view of frequency, it seems reasonable to suppose that, if the norm is what people use, the more frequently people include a foreign element in their speech, the more normal it is. How can we then determine the number of times people use a given word or form? One way of finding out is by counting the number of times that word or form comes up in suitable samples of speech or writing. Another way of determining the degree of integration would be by counting the number of texts in which a word occurs (range). Although the most frequent words are also those which occur everywhere (have the greatest range), some words are likely to occur in certain texts more than in others (Mackey, 1967).

Secondly, attempts have been made to suggest ways of finding evidence of integration into the code. These include tests of availability, acceptability and translatability. Availability is a measure of the potential of the items in a code. Whereas frequency of textual occurrence is a suitable measure of language forms which must be used, availability is the appropriate measure for words which may be used. These include the thousands of nouns, verbs, adjectives and adverbs which are more or less at the disposal of the bilingual speaker. How can we get at this store-house of vocabulary? One way is by asking the subject to supply an inventory. This can be accomplished through a formal test. This availability test has the speaker list items on each code according to any number of semantic fields. One way of measuring the availability of an item in a population is to take a representative sample of that population and have each person supply an inventory of the items in each conceptual or semantic field (for example, food, clothing, housing, etc). The type and number of conceptual fields depend on the detail and amount of information desired (Mackey, 1972c).

The acceptability test was suggested by Nils Hasselmo at the Unesco International Seminar on the Description and Measurement of Bilingualism (Kelly, 1969: 121-41), and later developed by him in a study of American-Swedish bilinguals (Hasselmo, 1969). The purpose of the test is to obtain a range of possible variation of selected items likely to occur in the normal, everyday speech of the bilinguals. The degree of acceptability of an item is indicated by the average score of a given group of subjects judging recorded sentences on a four-point scale. It is obtained by having each subject in a representative group of bilinguals rate constructed and actually observed test sentences containing elements from the other language. Each sentence is rated as to whether, speaking to a friend in the community, the subject would 'say it that way' almost always, sometimes, never (but others would), or never (and others neither). The results reported showed a complete range of degrees of acceptability.

Another access to the code of the bilingual may be had through a test of translatability, also suggested by Hasselmo. It tests the bilingual's ability to furnish equivalents in his other language. Testing procedures are similar to those used for obtaining indices of acceptability. Here the bilingual hears words from one of his languages in the context of the other and is asked to supply the equivalent form in the language of the text. The equivalent must cover essentially the same content as the test item. If the

bilingual is unable to supply a suitable equivalent, it may perhaps be assumed that the item actually belongs to his other code, or to both. For example, in the French sentence, *Je voulais enlever la roue, mais j'ai perdu mon wrench*, one bilingual tested was unable to find the French equivalent of *wrench*, insisting that it was after all a French word. Preceding translatability, Hasselmo has a test of identification to find out in which language the bilingual classified a selected group of items, identifying those which he thinks had been taken over from the other language. The results also showed a continuum, ranging from complete identification with one language to complete identification with another (Mackey, 1970b).

These tests are to be distinguished from the intelligibility tests used by Haugen (1966) and those used by Voegelin and Harris (1950). Students trying out the latter tests quantified the amount of information understood by people of one language listening to other more or less related languages. Correlations between the results of such intelligibility tests and the number of differences between languages have not been established. But the quantification of the latter has been of interest to students of the *Sprachbund* (Afendras, 1969). It is the kind of differential linguistics which has concerned students of communication areas interested in establishing 'degrees of variation between varieties within a system or between a variety of systems' (Neustupný, 1971).

It is also of concern to the establishment of measures of interference between similar and dissimilar languages and dialects. In a book on interlingual distance (Mackey, 1971) the present writer has suggested mathematical techniques for measuring and distinguishing the following: i) distance in the two codes and distance in the discourse produced through them, ii) distance as communication of static messages, in space, as distinct from different texts unrolling in time, iii) distances as differences between languages, as opposed to distance as movement from mother-tongue to other tongue, iv) distance as an analytic or taxonomic accumulation of categorical differences, or distance as an over-all integrated measure, v) distance as a direct measure of difference, in contradistinction to indirect measures making use of an intermediate language, vi) distance as the diversity, intensity and productivity of comparable differences between the languages. Since not all these different types are mutually exclusive, the distance between two languages may be measured from a number of different angles. What do the different types of distance involve?

All languages have systems of symbols variously referred to as *langue* or code, and these are used in conventional ways to produce messages or tests - an activity which has been called by a number of names including *parole* and discourse. Measuring interlingual distance from the one is not likely to be the same as measuring from the other.

In measuring the distance between Language A and Language X therefore we can proceed either from the descriptions of the languages or from samples of them.

Measurements of the differences between two language codes depend on the accuracy and comparability of the descriptions used. If the information supplied is either incorrect or incomplete, for one or both languages, the consequent comparison or contrast is likely to be inaccurate. If the type or methods of description (and notation) used are not identical, differences in descriptive techniques may appear as alleged differences in the languages. That is why some of the contrastive studies which are based on the two codes have their source not in the use of ready-made descriptions but on redescriptions of the languages for purpose of comparison - or contrast. This is another reason why two contrastive descriptions of the same languages or dialects may differ.

In measuring from discourse samples, however, sets of equivalent texts with alternative versions are needed. But even with an inventory of all possible alternatives, it will be seen that measuring from the message is not the equivalent of measuring from the code. For one cannot assume that different codes will produce correspondingly different equivalent texts or that the degree of difference between two equivalent texts corresponds to degrees of difference in the codes which generate them. This is evident when one travels from one area to another area in which the same language is spoken. There are situations, for example, in which Englishmen say 'Not at all', Canadians say 'You're welcome', and Americans say 'Uh, uh'. Yet these three equivalent discourse responses to the same situation do not indicate that we have three different languages.

Between two different languages, the different codes may produce equivalent alternatives which are closer than others. For example, the English sentence *The name is Robert* may be rendered with equal correctness in French as either *Je m'appelle Robert* or *Le nom est Robert*, the latter being much closer to the English equivalent. This does not mean, however, that the two French sentences are interchangeable; they are, on the contrary, linked to mutually exclusive situations, the first being used in introduction, the second in consultation and verification. The English sentence is used in both; to do so in French would consequently constitute a case of interference. Measuring language differences through equivalent texts therefore is valid only where both texts operate within the same context (Mackey, 1974).

Similarities in code do not necessarily produce similarities in discourse. When measuring from the code, the English and German vocabularies may turn out to be very close, as it is in the German sentence: *Kann* (can) *man* (one) *ein* (one) *Boot* (boat) *haben* (have)? But measuring from the English equivalent of this sentence, the two languages no longer seem so similar:
G *Kann man ein Boot haben?* / E *May we rent a boat?*

Although such code similarities may facilitate comprehension of the other language, they may be a source of interference in expression. The codes of the two languages may be close, while their respective transformations into equivalent messages may be highly divergent.

Contrariwise, the identity of two equivalent messages does not mean that the codes producing them are likewise identical. Sentences like *Maria va a casa* may be read as either Spanish or Italian. But if one were to compare the two codes which produced this same sentence, the two languages would appear as different: S (pres ind): *voy, vas, va, vamos, vais, van* / I (pres ind): *vado, vai, va, andiamo, andate, vanno.*

Spanish	Italian
Yo *voy* a casa	Io *vado* a casa
Maria *va* a casa	
Nosotros *vamos* a casa	Noi *andiamo* a casa
Teresa y Maria *van* a casa	Teresa e Maria *vanno* a casa

The distance between two languages, therefore, is unlikely to be the same when measured from their codes as when measured from their messages. We must conclude that neither measure is alone sufficient to give the complete picture; we require both. And since they are so very different, we must consider measuring separately distance between codes and distances in discourse.

Distance between languages will also be different depending on whether we measure the static results of language communication or the dynamic process of the generation of texts from their respective codes.

In static measurement, language is considered as if it were a reservoir of material to be matched and measured. Two languages may be treated as if they were two landscapes, each corresponding feature of which is given equal consideration. Language can also be regarded as a kinetic process, the dynamic generation of texts on a never-ending time scale. And the point on the scale at which elements of languages are equivalent can be regarded as a function of their difference.

Since discourse is essentially sequential, it can be regarded as unrolling in time, so that at no given time is a whole text perceptible. In this way, the resemblance of what comes before makes us anticipate what may come after. And because of this, the matching elements that come first have more resemblance value than those that appear later. For the probability of one unit along the time sequence being identified with its counterpart depends on the type and number of elements that have already appeared.

Since the sequences of phonemes appear, not in space but in time, the position of a phoneme in one of these temporal sequences can be weighted. If, instead of reading and translating the texts, one were to listen to them, as do interpreters, the similarity of elements at the beginning of words or word groups becomes more important than it does at the end. This importance is also reflected in the natural tendency to form oral abbreviations like *math* and *gym* - rather than **matics* and **nastics*. The operative factor, however, is not sound, but sequential position. Some computerized systems are able to operate - even with proper names - with no more

than five letters per word - provided they start counting from the first letter. It is almost as if a series of pictures were to appear in sequence, giving us more and more information as the film unrolls. In this sense, language is essentially kinetic, and if all its characteristics are to be measured, this must be taken into account (Mackey, 1974).

We may regard the distance between languages either as the sum of their differences or as the amount of change needed to convert one language to the other.

In adding the differences between two languages, one may conceive them as opposing forces, or vectors, which can be combined as a measure of divergence (differential or vectorial distance). Or one can regard these differences as the amount of work or number of operations necessary to convert one language into the other (conversion distance). The first, being a combination of forces, is non-directional; the second, being a transfer of operations, is uni-directional. Let us examine what the measurement of each involves.

The measurement of the differential distance between Language A and Language X, being non-directional, is obtained by the summation of differences between both languages, so that $D(A \curvearrowright X) = (A - X) + (X - A)$. For example, in measuring the difference in the system of determiners in French and its counterpart in English, the fact that the single indefinite article (*a*) is equivalent to the two of French (*un, une*) is the same amount of difference as the fact that the person determiner in English (*his*) is equivalent to two forms in French (*son, sa*), a difference of one in each case. To say that the English (*his, her*) = French (*son, sa*) and count a distance of zero would be to ignore the fact that (*her* = *son, sa*) - (1-2) and (*his* = *son, sa*) - (1-2), and (*son* = *his, her*) - (2-1), (*sa* = *his, her*) - (2-1). So that the distance is not zero, but rather, in natural numbers: [1-2] + [1-2] + [2-1] + [2-1] = 4, that is an absolute, or vectorial, difference of 4. The vectorial distance between the series of singular determiners in English and their French equivalent will then be a total of 13, calculated as follows:

TABLE I

F	un une	le la	ce cette	ce cette		ce	mon ma	ton ta votre	son sa	son sa	son	sa
E	a	the	this	that	this that	this that	my	your	his	her	his her	his her
F	2	2	2	2	1	1	2	3	2	2	1	1
E	1	1	1	1	2	2	1	1	1	1	2	2
F∿E												
13=	1 +	1 +	1 +	1 +	1 +	1 +	1 +	2 +	1 +	1 +	1 +	1

In other words, if we add the differences, between English and French, in the distinctions recognized in this series of determiners with those between French and English, we get a total of 13. If we were to add all such differences in all the semantic and formal elements of these two languages we would get an idea of how far apart they are. But we would not know how many more distinctions would have to be made by a Frenchman learning English in comparison with those which an Englishman would have to make if he were learning French. Here the starting point would make a difference, since the distance between A and X would not be equivalent to the distance between X and A. We would have to know which language features are being converted into which.

It may make a practical difference whether a bilingual starts off with a native language that makes a large number of compulsory distinctions and tries to master one that does not, or whether he is going from a simple language to a more complex one. Levensten (1966) has clearly demonstrated the difference that direction can make. In order to find out this practical difference, differential measures would not be sufficient. What is needed is an idea of how many extra distinctions have to be made when passing or converting from one specific language (Language A) to another (Language X); in other words, we would have to measure the conversion distance. Morphological difference between two languages has been described as the number and content of grammatical instructions needed to generate the utterances of one language out of the utterances of the other (Harris, 1954).

The measurement of conversion distance, being uni-directional, is obtained by computing the number of distinctions not present in the other language. In the above example of the set of singular determiners in English and French, it will be seen that for every English determiner there are at least two French ones, one masculine and the other feminine; so that when converting from English into French, there is additional distinction of gender each time. In addition to this, there is in one determiner (the second person singular) an extra social distinction to be made between the general and the familiar forms. The conversion distance is the sum of these distinctions that do not exist in the source language; difference between the language to be attained (the target language) (T) and the language already acquired (the departure or source language) (S). So that, S → T = T − S = A − X.

TABLE II

A(F)	un une	le la	ce cette	ce cette	mon ma	ton ta votre	son sa	son sa
X(E)	a	the	this	that	my	your	his	her
A	2	2	2	2	2	3	2	2
X	1	1	1	1	1	1	1	1
A − X=	1 +	1 +	1 +	1 +	1 +	2 +	1 +	1

$$D_{sg.dt} \quad (E \rightarrow F) = 9$$

These represent distinctions of only two types, one (masculine-feminine) with the highest productivity, covering all or almost all determiners and nouns, and the other (*ton-ta, votre*) limited to the second person singular determiner and pronoun.

Measurement in the other direction, however, gives quite a different picture. The French speaker learning English has to make only a few extra distinctions he does not make in his French determiners, but these are in different semantic areas. So that, F → E = E − F.

TABLE III

A(E)	a	a	the	the	this that	this that	my	my	your	your	his her	his her
X(F)	un	une	le	la	ce	cette	mon	ma	ton	ta	son	sa
A	1	1	1	1	2	2	1	1	1	1	2	2
X	$\frac{1}{0}$	$\frac{1}{0}$	$\frac{1}{0}$	$\frac{1}{0}$	$\frac{1}{1}$	$\frac{1}{1}$	$\frac{1}{0}$	$\frac{1}{0}$	$\frac{1}{0}$	$\frac{1}{0}$	$\frac{1}{1}$	$\frac{1}{1}$
A-X =	$\frac{0}{0}$ +	$\frac{0}{0}$ +	$\frac{0}{0}$ +	$\frac{0}{0}$ +	$\frac{1}{1}$ +	$\frac{1}{1}$ +	$\frac{0}{0}$ +	$\frac{0}{0}$ +	$\frac{0}{0}$ +	$\frac{0}{0}$ +	$\frac{1}{1}$ +	$\frac{1}{1}$

$$D_{\text{sg.dt}} (F \rightarrow E) = 4$$

A French speaker of English would have to make extra distinctions which he does not make in French, and each of a different type, the first having to do with proximity and the second with sex. They are high intensity items, however, because of their frequency of occurrence but since they apply to small classes of words, their item productivity in the code is lower (see below).

One can also measure the distance between languages either by taking the differences category by category, that is, taxonomically, or by attempting to abstract from them integral measures in order to obtain an over-all picture.

In the taxonomic measures we have to deal with classes, categories or dimensions. Each has to be considered separately and measured by a procedure appropriate to the dimension being analysed. For example, differences in syntactic relationship such as the concord between adjective and noun, may not be measured in the same way as the difference in class membership where the item may be an adjective in one language and a noun in the other (Mackey, 1970a).

In the integral measurement of interlingual distance, only the equivalents as a whole, and only as they meet the eye or the ear, are measured for their observable differences. In the first case, the results of the measurement appear as a sequence of figures each representing the distance in the category or dimension included. In the second, the distance appears as a single integrated figure. Mathematic formulæ of integration have also been elaborated (Mackey, 1976).

Studies of language have kept separate the description of what is said from that of how it is said. The dichotomy has variously been called semantics vs formal and content vs expression. Without

denying that all language features, even those of meaning, are formalized in the code, let us follow the conventional usage by calling what is said, the content form, 'semantic', and how it is said, the expression form, 'formal', for want of a better term.

It will be seen that formal differences between languages are not equivalent to semantic differences. A look at a few deceptive cognates will make this clear. For example, the form *location* is the same in written French as it is in written English. In the semantic code of French, however, it does not cover the same area of meaning as it does in English, since it would sometimes have to be rendered in that language as *rentals, to let*, and other forms. An examination of the semantic equivalents in English and French of such words as *administration, information, relation, transmission, page, machine* - to name only these - would reveal the same distinction between formal identity and semantic divergence.

The distinction, therefore, may be an important one, since the grouping of concept categories is not necessarily connected with the relation between word forms. The fact, for example, that languages group words into masculine and feminine does not signify that the difference has anything to do with the meanings of these words.

In comparing language codes, therefore, the distinction between semantic distance and formal distance will have to be maintained. In measuring the distance in discourse, however, the distinction no longer holds, since the comparable texts being equivalent, the semantic component is a constant. Interlingual distance may be direct or indirect. If the difference between two languages or dialects can be measured directly, there may seem to be no point in proceeding otherwise. In certain studies, however, where interlingual measurement may be necessary, the only significant differences may be indirect ones. For example, if we wish to measure the difference in standard of the two languages in a bilingual community , we will not find the answers by comparing the one with the other, but rather measuring each against its standard regional or national forms.

Or in the study of two mixed languages with a common parent, we can find out by indirect measurement the extent of divergence of each and the results of the different types of mixture.

Measurement of interlingual distance may therefore be done either directly by comparing one language with the other, or indirectly by using an intermediary language or dialect, as is sometimes necessary in practice.

In attempting to measure the distance between languages, we may wish to know, not only how different the languages are, but also how important are the differences, both to their users and to the working of the languages themselves.

The distance between any two comparable sets of features in two languages may vary in three respects - in diversity, in intensity and in productivity. Diversity has to do with the number of distinctions a language makes in its grammar, its vocabulary and its phonetics. Intensity is a measure of the importance of such

distinctions. Productivity shows the number of areas (situations, structures and categories) to which the distinctions apply. Distance between languages can be measured in terms of the differences in diversity, intensity and productivity of their comparable elements.

Differences in diversity can be measured by subtracting the number of constituents of one language from their counterparts in the other. For example, in the semantic area of the third person singular pronouns, the difference between English and French is 1: (*he, she, it*) - (*il, elle*) = 3 - 2 = 1 (Mackey, 1972c).

Differences in intensity can be measured by the use of the appropriate scale of importance, depending on whether the comparable units are discourse features like the interconnectives (*although, since, if,* ...) logical abstracts (*be, seem,...*) or environmental variables (*wine, snow*). Appropriate scales may be found in the indices of frequency, range, coverage, availability and familiarity (Mackey and Savard, 1967; Mackey *et al*, 1971).

Productivity has to do with the degree of generality at which the distinction is actually applied. Does it affect a large class of elements (like all sentences, or all nouns), or only a small class? Within the class, does it affect all members or only a few (as do the *foot-feet, goose-geese* plurals in English)? French, for example, makes action on self vs action outside self a compulsory distinction, whereas English permits a choice (F *Je me lave* / E *I wash, I wash myself*). This potential of difference in compulsory distinctions can be multiplied by the number of items so affected. Differences in productivity can also be regarded as the number of structures or units into which an element can fit. For example, in the formation of plurals by vowel mutation (eg E *mouse-mice* / G *Maus-Mäuse*), German is more productive than is English, where the plurals are usually in *-s*. In this respect, English is closer to Spanish than it is to its Germanic cousin. Productivity differences, therefore, can be measured by counting the number of items in each language affected by the difference, and by calculating the number of possible utterances which the distinctions may produce (Mackey, 1969).

In sum, in measuring the extent to which different languages differ, we must distinguish between what we are measuring and where we are measuring it. What we are measuring may be the differences themselves, or the amount of work needed to change one language into another (differential and conversion distances). Where we do the measuring may involve us either in comparing code items or in analysing samples of speech (code and discourse distances). The techniques of measurement used will depend on the types of distances measured.

In the study of language contact it is not sufficient to be told that an element of one language does or does not exist in another language. We must know the extent to which elements have been integrated and this necessitates a certain amount of quantification. It may be that in the study of interlingual differences the most useful language of all may be the most universal of all - the language of number.

REFERENCES

Afendras, E A
1969 Can one measure a Sprachbund? (Publication B-22), Quebec: International Center for Research on Bilingualism. Reprinted, Folia Linguistica 4: 87-99.

Clune, M G
1967 Transference and Triggering: Observations on the Language Assimilation of Postwar German-speaking Migrants in Australia, The Hague: Nijhoff.

Darbelnet, J
1970 'Dictionnaires bilingues et lexicologie différentielle', Languages 19: 92-102.

1971 'La linguistique différentielle', Meta 16: 1-2.

Di Pietro, R J
1971 Language Structures in Contrast, Rowley, Mass: Newbury House.

Filipović, R
1971 'The compromise system: A link between linguistic borrowing and foreign language learning', in IRAL -Sonderband: Kongressbericht der 3. Jahrestagung der Gesellschaft für angewandte Linguistik, Heidelberg: Groos, 197-206.

Gilbert, G G (ed.)
1970 Texas Studies in Bilingualism, Berlin: de Gruyter.

Grünbaum, M
1885 'Mischsprachen und Sprachmischungen', Sammlung gemeinverständlicher Vorträge 20: 1-48.

Harris, Z S
1954 'Transfer grammar', International Journal of American Linguistics 20: 259-70.

Hasselmo, N
1969 'On diversity in American Swedish', Svenska Landsmål och Svenskt Folkliv: 53-72.

Haugen, E
1950 'The analysis of linguistic borrowing', Language 26: 210-31.

1956 Bilingualism in the Americas: A Bibliography and Research Guide (Publications of the American Dialect Society No 26), Alabama: U.P. Reprinted 1964.

Haugen, E
1966 'Semicommunication: The language gap in Scandinavia', Sociological Inquiry 36: 280-97.

1973 'Bilingualism, language contact, and immigrant languages in the United States: a research report 1956-1970', in T A Sebeok (ed.) Current Trends in Linguistics 10.1, The Hague: Mouton.

Kelly, L G (ed.)
1969 Description and Measurement of Bilingualism: An International Seminar, University of Moncton 1967, Toronto: U.P., for UNESCO.

Le Page, R B
1968 'Problems of description in multilingual communities', Transactions of the Philological Society: 189-212.

Levenston, E A
1966 'A classification of language differences', International Review of Applied Linguistics 4: 199-207.

Mackey, W F
1953 'Bilingualism and linguistic structure', Culture 14: 143-49.

1962 'The description of bilingualism', Canadian Journal of Linguistics 7: 51-85. Reprinted 1968, in J Fishman (ed.) Readings in the Sociology of Language, The Hague: Mouton, 554-84.

1965 'Bilingual interference: its analysis and measurement', Journal of Communication 15: 239-49.

1966 'The measurement of bilingual behaviour', The Canadian Psychologist 7: 75-92.

1967 Language Teaching Analysis, Bloomington: Indiana U.P. First edition 1965, London: Longman. New French edition 1972, revised and edited by Laforge as Principes de didactique analytique, Paris: Didier.

1969 'La rentabilité des minilangues', Cahiers Ferdinand de Saussure 26: 49-64.

1970a 'An algebra of language differences', Actes du dixième congrès international de linguistes 3: 611-21.

1970b 'Interference, integration and the synchronic fallacy', Georgetown Monograph Series on Languages and Linguistics 23: 195-277.

Mackey, W F
1971 La distance interlinguistique (Publication B-32), Quebec: International Center for Research on Bilingualism.

1972a International Bibliography on Bilingualism, Quebec: Laval U.P.

1972b 'Dialinguistic identification', in E Firchow *et al* (ed.) Studies for Einar Haugen, The Hague: Mouton, 349-60.

1972c 'Concept categories as measures of culture distance', in S K Ghosh (ed.) Man, Language and Society, The Hague: Mouton, 134-68.

1974 'Les dimensions de la linguistique différentielle', Le français dans le monde 103: 25-31.

1976 Bilinguisme et contact des langues, Paris: Klincksieck.

Mackey, W F and J-G Savard
1967 'The indices of coverage: a new dimension in lexicometrics', International Review of Applied Linguistics 5: 71-121.

Mackey, W F, J-G Savard and P Ardouin
1971 Le vocabulaire disponible du français (2 vols), Paris: Didier.

Nemser, W J
1961 The Interpretation of English Stops and Interdental Fricatives by Native Speakers of Hungarian, New York: Columbia U.P.

Neustupný, J V
1971 'Toward a model of linguistic distance', Linguistic Communication 5: 115-32.

Oller, J W and J C Richards (ed.)
1973 Focus on the Learner, Rowley, Mass: Newbury House.

Rheinfelder, H
1926 Vergleichende Sprachbetrachtung im neusprachlichen Unterricht, Munich: Heuber.

Rozencvejg, V J
1972 Kontakty Jazykovye: Lingvističeskaja Problematika, Leningrad: Akademija Nauk SSSR.

Sandfeld, K
1930 Linguistique balkanique: problèmes et résultats, Paris: Champion.

Schuchardt, H
1872 'Albanisches und Romanisches', Zeitschrift für vergleichende Sprachforschung 20: 241-302.

Voegelin, C F and Z S Harris
1950 'Methods for determining intelligibility among dialects of natural languages', Proceedings of the American Philosophical Society 95: 322-29.

Wandruszka, M
1971 Interlinguistik: Umrisse einer neuen Sprachwissenschaft, Munich: Piper.

Weinreich, U
1953 Languages in Contact: Findings and Problems, New York: Linguistic Circle. Reprinted 1963, The Hague: Mouton.

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ROMANCE HISTORY - CREOLIZATION AND DECREOLIZATION? OR DIFFUSION AND FOCUSSING?

A disturbing, though salutary, effect of working with Bob Le Page - as I have done for over twelve years - is that one has constantly to re-examine one's most treasured prejudices and to discard one's favourite dogmas. The standard textbook examples are not merely to be passed on to students but carefully scrutinized and, often, found not to prove what they are supposed to. The very subject matter of linguistics is questioned: a 'language', in its different meanings, either remains a complete mystery (Language *ONE* and *FOUR* for Le Page, 1975) or is a theoretical construct (Language *THREE*) or seems to be more determined by extra-linguistic factors than most of us would like to admit (Language *TWO*).

Conditioned as I am more by Continental European linguistics than by American, I have always shared with Le Page a preference for dynamic linguistic models, especially those that take account of social factors. However, I have found it difficult to abandon the 'myth' of the 'language' which, like 'culture', is supra-individual and has some degree of autonomy. Le Page's experience in creole studies allowed him to break away from the more traditional views of language as an institution, to see clearly that in some circumstances each and every speech act creates a sort of instant pidgin, though in others *focussing* will create stereotypes that reflect communal solidarity. Romance speakers have perhaps more sense of linguistic identity than most, with a highly focussed Language *TWO* (an over-developed sense of 'correctness' and a high regard for those whose Language *ONE* approximates closely to Language *TWO*). Yet, frown though the purists may, speakers frequently find it difficult to perform satisfactorily in the standard - the 'focussed' language. Where they can claim to be aiming at another target - an equally focussed 'dialect' - all is well; but all too often they are conscious of a deficiency in their performance compared with their model.

It is a commonplace to say of the greater Romance area that there are no dialect boundaries, but a continuum of mutual intelligibility; it is, of course, not true today, where most speakers habitually use - or try to use - the 'focussed' national language, rather than 'diffused' local dialects. Where 'dialect' still lives it too is 'focussed', so that a Catalan from Roussillon and a Gascon, a Walloon and a Picard speaker in Belgium, will claim not to be able to converse in their dialects (though if they *want* to, they can easily construct a pidgin - actually they prefer to use French). In fact, perhaps dialects continue to exist only because of their differentiating power - so that differences

rather than similarities are emphasized when dialects come into contact. However, it remains true that the Romance languages remain similar enough to each other - lexically and morpho-syntactically - to make them, if not exactly 'mutually intelligible', at least 'easy to learn', once the 'noise' of phonological differences is overcome. How much of their similarity is due to their common origin - the Latin spoken throughout the Empire? And how much is due to convergence - focussing towards Latin (decreolization?) or towards each other?

Many linguists have viewed the development of separate Romance languages as creolization *avant la lettre*. They argue that the Latin of each part of the Empire must have been coloured by the substratum: for heavy 'colouring' to have survived the Imperial period implies a submerged Creole-Latin-speaking class, for which there is however no evidence. More plausible is the suggestion that the invading barbarians who adopted a pidgin Latin were succeeded by a creole-speaking generation whose languages became 'focussed' away from Latin. A third suggestion, which accords better with the chronology, is that Latin remained as the Language *TWO* (the acrolect?) of the Western Empire, even after its administrative collapse, but with speakers' performance (the basilect?) deviating more and more from the model, until social change (feudalism?) motivated linguistic focussing in smaller communities (so that the 'basilect' supplanted the 'acrolect', which nevertheless remained as the High language in a diglossic society).

How far any of these processes would fit in with received definitions of 'creolization' I do not propose to discuss here. I am more concerned with 'decreolization': can the convergence observed during the history of Romance (ie since the 9th century) be seen as a striving towards the Latin model, or is some other process at work?

It can hardly be denied that modern Romance vocabulary owes a great deal to Latin borrowing, though inter-Romance borrowing is far from negligible. As much of the vocabulary of any one Romance language is transparent (neglecting phonological differences) to speakers of another, this is an important factor in assessing similarity. I have shown elsewhere (Posner, 1974) that the Romance languages can be demonstrated to share the same phonological rules: certain morphological convergences can also be demonstrated (the 'Romance future' is a notable example).

Here, however, I want to concentrate on one small section of syntax, which has in recent years received an inordinate amount of attention from linguists, mainly because it is difficult to account for in classic TG terms. This is the question of 'special clitic' (cf Zwicky, 1977) pronouns and their ordering.[1] The parallels in the modern Romance languages are difficult to assign to a Proto-Romance stage and certainly do not reflect Latin usage. In general, the clitic object pronouns of Romance precede the verb (unlike object nouns) and are subject to rather rigid ordering in relation to each other. Most of the discussion has centred round the question of

whether the clitic node is introduced by phrase structure rules or by movement transformations (cf Emonds, 1975) and that the ordering is determined by surface structure constraints (Perlmutter, 1970) rather than by deep structure features. Also very much bound up with the question are the controversial 'reflexive' pronouns, which have diverse functions in the Romance languages. Indeed, Emonds suggests that a clitic node in deep structure is justified by the 'inherently reflexive' verbs.

All the evidence suggests that the modern state of affairs dates from the late medieval period at the earliest and that similar developments have taken place independently in different languages. Are we to suppose that certain universal tendencies have been at work? Although counterparts can be cited in other languages it is hard to believe that the rather remarkable Romance clitics reflect such tendencies. Or is there a Romance 'drift' that accounts for the development? A more down-to-earth treatment would be to account for it in terms of 'projection, diffusion and focussing' in Le Page's terms. The interaction between the Romance languages is well recognized, and even reconstructionists, like Dyen, shrug off the obvious convergences by talking of 'collusion' between dialects with catenate mutual intelligibility. What more natural than to seek the explanation of the special clitic problem in these terms?

There is certainly no evidence that it can be projected back into 'Proto-Romance'. Meyer-Lübke's 1897 suggestion, based on the evidence of early Romance texts, is that in the spoken Latin of the Empire atonic object pronouns became enclitic to the first stressed word of the clause, similar to the situation reconstructed by Wackernagel(1892) for Proto-Indo-European. It would be easy to ascribe this development to the interference of other Indo-European languages with which Latin came into contact, though it is by no means confined to Indo-European. What almost certainly can be assigned to this stage is a change in sentence rhythm that can account for distinction between tonic and atonic pronouns, with certain phonetic consequences: in Zwicky's terms there developed 'simple clitics', which were not phonologically nor syntactically distinct from their non-clitic counterparts, but which were phonetically abbreviated and which gravitated towards the least stressed positions in the sentence. When, and how, then, did the change from 'simple clitic' to 'special clitic' take place? The answer will obviously take into account phonology - when did the atonic and tonic forms cease to be derived by phonological rule from the same underlying form? - and morphosyntax - when was the tonic/atonic distinction overlaid by the disjunctive/conjunctive distinction?

First, it should be noted that all Romance languages do not have the clitic/non-clitic pronoun distinction: Surselvan in Switzerland uses postposed object pronouns to replace pronominalized nouns, just as 'Romance' creoles do. However, the evidence of earlier (17th century) texts suggests that proclisis was once a possibility, and even today the reflexive pronoun is proclitic to the verb (Prader Schucany, 1970).[2] Older Rumanian texts (from the 16th century) use postposed disjunctive pronouns: today, 'double-mention' - 'copying' by a proclitic pronoun (in dialects also an

enclitic pronoun) of a noun or disjunctive pronoun - is normal. In certain conditions the same is true in standard Italian and Spanish; in non-standard languages 'double mention' is especially frequent. This would suggest that the clitic object pronoun is an inherent part of the VP and that the object noun can optionally be deleted: Diez (1876) seemed to have this in mind when he tentatively suggested a parallel with Basque.

Secondly, there are some difficulties in viewing even early Romance pronouns as 'simple clitics'. Problems arise especially with the plural forms: in French, for instance, *nous* and *vous* appear phonologically to be protonic forms, whereas *leur* is tonic; *eux* must have developed from a differently accented form from *les* etc. Some of the formal differences must have arisen after the change from 'simple' to 'special clitics' in any case - for instance the I pl *ci* (possibly originally dative) alongside disjunctive *noi* in Tuscan; the II pl *os* contrasting with *vosotros* in Spanish etc. *Ci* appeared in texts from the 13th century onwards; *os* and *vosotros* are post-medieval. One puzzle is how in Italian *me* is tonic, and *mi* atonic, whereas in Spanish (and non-Tuscan Italian) it is the other way round. Is this a result of different phonological rules or are they haphazardly derived from *ME* and *MIHI*? If the latter, *mi* /*me* never were 'simple clitics'. On the other hand, in some languages like Portuguese it could conceivably be claimed that the clitics are still 'simple' phonologically: the fact that in European Portuguese atonic pronouns can still be enclitic, as in the medieval languages, may indicate that this conservative language has not shared in the 'Romance' evolution (in Brazil, on the other hand, proclisis is normal).

Thirdly, the 'clitic question' is complicated in Romance by the fact that the 'pronouns' sometimes have no obvious co-referential function: not only is the function of 'reflexives' difficult to define, but great use is made, especially in popular style, of 'dative-of-interest' pronouns (especially I and II sg) and, except in Surselvan and in modern Spanish and Portuguese, of the so-called 'adverbial pronouns' (from the locatives *IBI* and *INDE* mainly, for Kayne pro-PPs), used sometimes co-referentially, sometimes as locatives and sometimes with no precise function at all.

What I shall be maintaining is that the Romance clitics are more meaningfully treated in terms of diffusion of features between languages, of focussing towards a precise, defined, morphological grammar on the Latin model, and of analogy and blending at the surface level, rather than in terms of deep structure features and of transformations. In other words, clitic pronoun ordering looks a mess, because it *is* a mess, and the various strands can be most easily unravelled by looking at how they got tangled in the first place.

The question, I think, subdivides easily into two - the second presupposing the first: (1) how did proclisis develop? and (2) how did rigid ordering of a proclitic pronoun array develop?

As I have already said, proclisis does not seem to have been the rule in medieval texts, though where the finite verb occupies

the second stressed position in the clause, as it frequently does, the object pronoun precedes it. Where the verb is first, presumably to secure emphasis, the pronoun usually follows: this is true today in European Portuguese and is still possible in literary style in Italian and Spanish. Ramsden (1963) tracing the evolution in French and Spanish texts finds that proclisis was fully established in French by the 14th century, with Spanish lagging far behind: Green (1976) argues that proclisis became normal in Spanish by the 16th century. In Italian the old state of affairs is described in the first grammar (1495); but probably at this period it was already a literary device. Sorrento (1950) claims that the *verb + pronoun* order is more emotive, citing examples from modern dialects, where enclisis is used more with the historic present and with the disjunctive (emphatic) pronoun subject. This accords with the suggestion (eg Lerch, 1940) that there has been a change of rhythm from descending to ascending: the resistance to proclisis of positive imperative sentences (where the normal rhythm remains descending) and the fact that sentence-initial atonic pronouns appeared first in early 13th century French texts in polar interrogative sentences (where the rhythm was probably ascending) also seem to square with this hypothesis.

But how is it that all the 'major Romance languages' (according to Green, 1976 - though he makes no reference to European Portuguese) have developed in the same way? Green contests the view (cf eg Talmy, 1971; Santangelo and Vennemann, 1976) that the 'original' Romance word order was S - Comp - V, and that pronouns (and incidentally verb-endings) are modern inertial traces of an old system. He suggests a tendency in Proto-Romance for pronouns to cluster round the verb (surely a fairly universal one?) and cites the development of the Spanish future as a possible parallel of the uncharacteristic creation of an OV order (I, like him, am not convinced by the parallel, especially as the so-called 'Romance future' is a good candidate for a 'diffusionist' hypothesis of the type I am expounding here).

The obvious solution (for those not dogmatically opposed to 'convergence' as an explanation of some language changes) is that the 'proclisis' habit has spread from French where it is first attested during the 13th century. At around this period, there is evidence that the earlier heavy word-stress had given way to a sentence-stress, of the modern French type. Why this should happen is still a mystery: 'decreolization' after the phonological havoc caused by interference from Frankish? or, as Straka (1965) suggests, a result of physiological changes caused by malnutrition (he dates such changes from the period of the Hundred Years War)? or absence of loud-mouthed barons at the Crusades? Fantasy apart, one of the indirect consequences could have been the increasing use of atonic subject pronouns preceding the verb - a development which occurred more or less contemporaneously with more frequent proclisis of object pronouns and 'adverbial pronouns'. From the end of the 13th century the old tonic object pronouns begin to be used in texts as disjunctive subject pronouns, suggesting that at this period little trace remained of any 'simple clitic' system. Thus from this period a 'special' verb-proclitic array became normal - even at times with 'double mention' (especially at first in questions).

Though the change in French may have been phonologically motivated, the same is not true of the other Romance languages. How is it that

they have 'drifted' (or been towed?) after French?

If French influence is to blame, it probably occurred from the late 12th and early 13th century onwards. It is not too fanciful, I hope, to suggest that such influence was exerted not only by the literary language but also via the *lingua franca* of the Crusaders (cf Kahane, 1976), a Romance pidgin in which, at the early stage, French played a prestigious role. A text believed to originate in 1204 during the siege of Constantinople, quoted by Kahane, shows only preposition of pronouns (and abundant use of the so-called 'passive' or 'middle' reflexives, characteristic of Romance, but which possibly developed later in French than in Italian and Spanish - cf Togeby (1974) who quotes no French example before the 15th century). I suggest that during this period diffusion between the Romance languages took place, with returning Crusaders taking back to their homelands syntactic habits picked up while using the *lingua franca*. Such habits might well be regarded as 'colloquial' and so would penetrate only slowly into the conventional literary languages.

The geographical progress of proclisis through literary Romance squares with the hypothesis. It was adopted first by Provençal and Catalan (possibly from the 14th century - Ronjat, 1937-41), more slowly by Italian and Spanish (15th-16th centuries) and even more slowly by Portuguese (modern times). It is true that French prestige has long been great in Portugal, but it dates from more modern times and so perhaps Portuguese escaped the late medieval 'proclitic drift'. Brazilian Portuguese, I repeat, prefers proclitic pronouns - perhaps influenced by Spanish, whereas in Europe the Portuguese standard has, almost by definition, been differentiated from Spanish since the 16th century (Figueiredo (1928) pours scorn on this hypothesis and suggests that Brazilian usage is creolized). Another possible explanation for the difference between Brazilian and European Portuguese concerns intonation: in Brazil pronouns are pronounced with more stress than in Europe and consequently do not suffer phonetic reduction to such an extent (Vasquez Cuesta and Mendes da Luz, 1961).

The earliest Rumanian texts (16th century) use accented pronouns in preference to atonic, and postpose reflexives. Rosetti (1966) suggests Slavic influence. French influence was felt from the 17th century, and especially from the mid-19th century when the standard language was defined. In modern Rumanian, I repeat, 'pleonastic' use of clitics (ie 'double mention') is normal.

The non-literary languages - like Sardinian and Rhaeto-Romance - have been influenced in their turn by Italian and Spanish, and have, on the whole, adopted proclisis.

How far each language has gone along the road may depend on the strength of the French influence. French itself uses postposed pronouns only in positive imperative constructions. Other standard languages also postpose pronouns to non-finite verb-forms (with some variation chronologically and geographically). In non-literary languages conditions vary - in pre-war Occitan (Ronjat, 1937-41)

for instance, vestiges of postposition to non-finite verb forms were retained only in the extreme SW and SE. The Catalan of Roussillon has a usage like that of French, whereas other Catalan dialects (including the standard) are closer to Spanish. Rhaeto-Romance also varies from the extreme of Surselvan, with postposed tonic pronouns, to Friulian which still sometimes uses atonic enclitics (Gregor, 1975). Mainly, Italian usage is followed, but the Val di Fassa dialect is closer to the French model (Illiescu, 1972). Surprisingly enough, unlike Italian, the Nuoro (Sardinia) dialect reported by Pittau (1972; an account of pre-1926 usage) uses proclitic pronouns with the infinitive - though this may be motivated by an avoidance of an accumulation of clitics where an 'inflected infinitive' is used. In Old Sardinian texts (Wagner, 1938), object pronouns are enclitic to the infinitive as well as to the gerundive.

The picture accords well with the suggestion that an originally French usage spread unevenly to other Romance languages. We might compare with the development in English where preposition of pronouns was still found in the 15th century, though the trend from the 12th century had been towards postposition (Fries, 1940): a 'special clitic' series never developed. It is to be noted that in 13th century Anglo-Norman the phonetic distinction between tonic and atonic III pronoun object forms was apparently lost (Pope, 1934).

Influence via the *lingua franca* may not have been only one-way: perhaps the development of 'middle reflexives' in French was encouraged by interaction with other Romance languages, whence they had penetrated into Crusaders' pidgin. Besides clearly 'reflexive' and 'reciprocal' uses, the reflexive forms are used in Romance apparently to express gradual or inchoative action as well as a 'middle' function. *Se/si* forms also mark non-specified agents: they have attracted a great deal of attention lately, because they present a puzzle for synchronic description (eg how many deep structure morphemes does a surface structure *se/si* represent?). That such extension of 'reflexive' usage is pan- or Proto-Romance is suggested by the early Rumanian abundant use of reflexives, sometimes ascribed to Slavic influence (Rosetti, 1966). The varied use of *se/si* forms leads to ambiguities which in some languages are resolved by differential ordering: thus in Friulian 'impersonal' *si* is proclitic, whereas 'reflexive' *si* is enclitic to the verb (Gregor, 1975), while in Brazil 'reflexive' *se* is proclitic and 'middle' *se* enclitic (Mattoso Camara, 1972). In modern Italian *se lo, se* is reflexive while the *si* of *lo si* is indefinite (in older texts *lo si* is used for both). In standard Spanish, on the other hand, *se,* whatever its function, precedes other clitic pronouns.

The question of the 'reflexives', however, does not concern us here, except as a possible example of 'diffusion' between the Romance languages at the time of the Crusades. It is to be remembered that the extension of the preposition of pronouns, or of the function of reflexives, would not be a radical change and might pass almost unobserved. Morin (1975) suggests, for instance, that a 'procliticization' rule that was optional in Old French become obligatory in Middle French, with the domain of the rule changing from the S in Old French to the

VP at the end of the 'Classical' period. I would suggest, however, that, although the syntagmatic consequences were almost negligible, paradigmatic change was considerable, with the clitic pronouns ceasing to be 'simple' substitutes for nouns.

The other aspect of the question that I wish to examine - the linear ordering of clitics in series - is quite another matter. I would suggest that *focussing* is more relevant in this case than *diffusion*: the similarity between the modern Romance languages may be more to do with the stereotypes speakers have constructed for themselves, than with interplay between the languages, though this too plays its part.

One thing to note about the ordering is that it is rarely functional and that therefore it would be surprising (to me, at least) if it were in practice as rigid as in the grammar books. In most Romance languages formal expression is given to case distinction (ACC v DAT)[3] in the III clitic pronouns, but not in I, II or REFL. Even in Rumanian where there is a (not wholly consistent) formal distinction between DAT (eg *mi*) and ACC (eg *mă*), the ACC I, II and REFL is not used in clitic sequences with the exception of *te*, of which more later. It appears that everywhere else in Romance I, II or REFL is interpreted as DAT in combination with III. Where two I, II or REFL clitics appear in sequence one of them (preferentially I) is interpreted as a 'dative of interest'. In Italian, however, some speakers can use a sequence of I,II clitics with ambiguous case function.

Another thing to note is that clitic sequences, with co-referential function, are avoided in the non-standard languages (even in standard usage, three may be the upper limit: Suñer, 1974).[4] However, there seems to be an affection for clitic arrays without precise co-referential function - sometimes the elements are analysed as epenthetic glides, or reduplications, as well as benefactives, locatives etc (cf Olszyna-Marzys, 1964). It is quite usual, almost everywhere, to omit a co-referential ACC III in combination with a DAT III, and not infrequently disjunctive pronouns are used, apparently to avoid the potential ambiguity of a clitic series.

Thirdly, the co-referential pronoun system was a happy playground for the grammarians whose task it was to establish standard languages to rival Latin, from the Renaissance onward. Here was one system in which morphological case distinctions really seemed to have survived, and here was a part of language in which logical relations could be expressed: to avoid ambiguity, precise rules were laid down and, to a greater or lesser extent, obeyed.

The best documented example of the grammarians' influence on clitic ordering is in French; during the 17th century in particular a battle raged about eg the *le vous/vous le* ordering, about the omission of *le* in the *le lui* series, about the restriction of *y* and *en* to non-personal reference, about the use of *le neutre* to refer to propositions etc (cf eg Brunot, 1905-53; Pinchon, 1972; Galet, 1971). By the beginning of the 18th century the battle was won, and it remained only to impose the usage on speakers - a goal now pretty well achieved.

In the other languages stereotyping has not been so efficient, and it is a mistake to say there is no variation. However, it is true to say that some orders are preferred to others and some explanation for this fact is called for.

Wanner (1974), in his account of the historical development of clitic series, deliberately simplifies the question by opposing two possible orders:

(1) ILLUM MIHI [III acc] - [III dat] - II, I

(2) MIHI ILLUM II, I - [III dat] - [III acc].

Nearly all modern languages, he says, use order (2), with some special deviations, while Catalan, Provençal, French and some Northern and most Central Italian dialects preferred order (1) until after the 13th century.[5]

By characterizing the pronouns as (functionally?) 'accusative' or 'dative' and by omitting reference to the 'adverbial' pronouns, some potentially important factors are left out of account. Whereas ACC and DAT can serve as pointers to the form, some other functional designations are preferred by some: Theban (1971) uses 'patient'/ 'beneficiary'; Szabo (1974) adds 'benefactive' to dative and accusative. Perhaps more satisfactory are García's functional labels:

LESS ACTIVE / LEAST ACTIVE

LOW DEIXIS / HIGH DEIXIS

which interact with other functions:

HUMAN / NON-HUMAN

MASCULINE / FEMININE

I, II, REFL / III

PRONOMINAL / ADVERBIAL.

The overlapping of functions can explain some apparent anomalies: HUMAN is not likely to be LEAST ACTIVE (eg I and II are understood as LESS ACTIVE in combinations); nor, for Spanish-speaking *machismo*, is MASCULINE (cf García, 1975). In standard Rumanian (though apparently now not commonly - Bredemeier, 1976) II sg, a socially unprestigious form, can occur as LEAST ACTIVE in combinations, whereas other I and II clitics cannot (*mi-te, le-te, si-te / ţi-l, ti-şe,* but **ţi-mă, *le-mă* etc). I and II and REFL are, on the other hand, less highly DEICTIC than III clitics.

Theoretically, one might expect proclitics to progress from left to right from MOST ACTIVE (agent) to LEAST ACTIVE, from LOW DEIXIS to HIGH DEIXIS, from HUMAN to NON-HUMAN, from PRONOMINAL to ADVERBIAL. Where, however, there is a formal distinction between ACC and DAT,

ordering need bear little functional load. In modern English, where there is no such distinction, the ordering LESS ACTIVE - LEAST ACTIVE is usual: *I giv̆ her it.* However, some doubt might arise with *I giv̆ her him*, while, for me, *I giv̆ it her* the NON-PERSONAL *it* would be more readily interpreted as LEAST ACTIVE, in spite of the ordering.[6]

Thus even in English ordering rules, though functional, are not necessarily completely rigid. In Romance, where function is rarely shown by ordering alone, they are, in principle, even less so. In some Spanish usage, however, the III DAT/ACC distinction is over-ridden by the HUMAN/NON-HUMAN one (complicated by MASCULINE/FEMININE as well), and so ordering may be used to signal case function. It is perhaps because it was functional that the Castilian order *se* (or *ge*) *lo* etc (DAT + ACC) seems to have been rigid from the beginning, (though this is by no means true of the less functional ordering *se me* etc, non-standard speakers all over the place use *me se*, as well). (I do not propose to discuss yet again modern Spanish *se lo*, *se me* etc: García's analysis of *se* as marking LOW DEIXIS rather than case function seems to me wholly convincing and covers so-called 'middle' and 'impersonal' uses - for the Italian counterpart cf Napoli, 1976 and Costa, 1975.) In Aragonese where III DAT is kept formally distinct (as *li*) from III ACC, the Castilian ordering restrictions do not apply. It is to be noted that the 'adverbial' pronouns survive here too, perhaps supported by Catalan, whereas further West they ceased to be used by the 15th century (Badía Margarit, 1947): this may be relevant, as the forms derived from IBI have frequently III DAT function in the Romance languages in which it has survived, whereas in Old Castilian they seem to have had only locative function and never to have been used clitically, in any consistent way.

In some other languages the order III DAT + III ACC seems to have always been preferred, usually accompanied by a preference for I, II, REFL + III, perhaps by analogy, where I, II and REFL are interpreted as DAT. In Portuguese, the *mo*, *to*, *lho* 'combinations' may be phonologically determined (CV) - in colloquial Brazilian, the III clitics are little used anyway (Mattoso Camara, 1972). In Sardinian, too, modern *liu* reflects a *li lu* (DAT + ACC) found in older texts, alongside *millu*, *nolla* etc (I, II + III), apparently with no counter-examples: it is to be noted that 'locative' *bi* can replace III DAT in modern Logudorian usage, only in combinations and then preceding the ACC (Wagner, 1938). In standard Rumanian (Acad, 1963; Olsen, 1928) the order DAT + ACC, I + II + III, is usual with *mi*, *ţi*, *si* (in popular style also *ni*, *vi*) used when followed by other pronouns. The I ACC forms *mă* and *vă* never occur in combinations. When two or more DAT clitics occur in series (eg *mi ţi-l ardea*) I and II are interpreted as 'ethic' or 'possessive' datives. As I have said, older texts use clitics comparatively rarely: French and Italian influence (Close, 1974; Poalelungi, 1973; Niculescu, 1971) operated at the time of the formation of the standard (19th century) and, in this matter as in others, the language was tidied up, and potential ambiguities resolved, by fixing the order of clitics.

It should be noted that in those languages that allow a combination of I, II and REFL clitics, the relative ordering of these is varied. Standard French, with an abhorrence for the 'ethic' dative, discourages any such combination;[7] in other languages a great deal of personal variation seems to be possible: Spanish, Portuguese and Catalan all 'prefer' *se* + II + I + III; Italian 'prefers' *mi* + *vi* + *ti* + *ci* + {*gli* / *le*} + *si* + *ne* + *lo*; Rumanian, I + II + REFL + III etc; but not all speakers agree.

What is remarkable is that some languages have, if we are to trust the textual evidence, changed their preferred clitic order, after the change from enclisis to proclisis came about. In France, East Iberia and North Italy older texts apparently show a preference for the order ACC + DAT and consequently(?) for III + I, II, REFL: it has been suggested (Melander 1929; Rohlfs, 1949) that this preference dates from regional Latin, though with what justification who can say? The older order still survives sporadically in parts of Southern France, especially where a distinctive III DAT is used: written Occitan, on the whole, adopted the French ordering system in the mid-18th century, a dating consistent with the hypothesis of French influence (Ronjat, 1937-41). Sometimes in Occitan the ⌜*me lou*⌝ type is differentiated from a ⌜*li me*⌝ type where the *me* is interpreted as an 'ethic dative'. Catalan today uses the Spanish order (though not invariably - Badía Margarit, 1951). When exactly the change came about is not known (the older ordering is found in Metge 1398 - cf Par, 1923), but it was almost certainly influenced by Spanish: the literary language, based on Valencian in this respect, is closer to Spanish than the colloquial - thus Valencian III DAT + III ACC *li'l, li la, li lo* is represented as *l'hi* elsewhere ([li], identical with the III DAT), while Valencian III DAT + ADVERBIAL *li'n* appears as *n'hi* etc (Solà, 1973).[8]

In French, the modern ordering is attested from the 14th century, but was not mandatory till the 18th. The III ACC + III DAT combination OFr *le li* is rarely attested, *li* being used alone (replaced by the tonic form *lui* from the later 13th century onwards). Modern non-standard still uses *lui* alone, but the standard imposes *le lui*: here possibly a SHORTER + LONGER phonological constraint operates, rather than a functional one. In Old French too, the III + I etc series was usual, possibly because enclitic *-l* was more readily used in such formulae as *Jel vos dirai*. The earliest examples of I, II + III order are of *tel* in the late 13th century when we assume that proclisis was regular. There was some resistance to the imposition of the modern order during the 17th century (cf Vaugelas, ed Streicher, 1934-6) but, to have triumphed, it must have been the most prestigious one. In the 'Franco-Provençal' dialects of the Valais, the Old French order is sometimes found (examples are rare as III ACC is often omitted, but *le me* is heard alongside *me le*). This is a measure of the resistance of these dialects to French: however, there are now virtually no speakers under the age of sixty, parents even speaking to their children in French. Thus, the remaining users of the dialects possibly focus away from French to preserve its identity, even though they are syntactically very close to French (cf Olszyna-Marzys, 1964).

It was thus the stereotyping of the standard that imposed the 'surface structure constraints' on French: but how did one of the possible orders of middle French become more prestigious? Here the only relevant ordering is the I, II, REFL + III one: the *en* + *y* / *y* + *en* is amply discussed by Pinchon (1972) and ordering relative to the infinitive by Galet (1971; phonological considerations may again be paramount here). I should maintain that the ⌜*me le*⌝ type of ordering may have gained prestige during the 16th century when Italian influence on the French court was strongest. It remains therefore to discuss how Italian came to prefer the ⌜*me lo*⌝ order to the ⌜*lo mi*⌝ one common in the earliest Tuscan texts.

Wanner (1974) discusses the Italian history in some detail: much of his information derives from Castellani (1952) who believes Tuscan was penetrated by influence from other dialects during the 13th century. By then the III ACC + III DAT combinations *lo li*, *la le* etc had levelled to *lili*, *lele*, and so were impenetrable to case analysis. The later development of these combinations (→ *glielo* in the 14th century → *glielo* etc in the 15th century) indicates reanalysis as DAT + ACC, which Castellani believes was due to the analogy of the ⌜*me lo*⌝ sequence which became common in 14th century literary Tuscan. The earlier sequence ⌜*lo mi*⌝ is last attested in texts in the early 15th century (enclitic *-loci* *-lovi* alongside proclitic *mel*, *ve lo* etc).

Castellani, following Melander, maintains that the (DAT + ACC) I, II + III sequence was always commoner south of Rome and in many Northern dialects: it had penetrated to Liguria, Piedmont, Bologna and Venice by the 13th century. Through Siena, Lucca and Pisa, it spread to Florentine (and hence was adopted by the standard).

That dialectal differentiation should be so clearcut is hard to believe.[9] Perhaps phonological factors favoured the early Tuscan ⌜*lo mi*⌝ sequence: not only is the vowel of I, II, REFL the somewhat puzzling *i*, compared with *e* virtually everywhere else, but also, as in Old French, the masculine III ACC easily adhered to a preceding element when enclisis was the rule (often *'l*, *il*, *el*, compared with *lo*, *lu* elsewhere). As for the conjunction of two III clitics, there is so much variation in form in dialects that it is hard to maintain that DAT + ACC order was ever rigidly maintained: evidence is in any case less readily available than for other combinations, in the early texts.

Castellani's examples of the ⌜*lo mi*⌝ sequence in 13th century Florentine prose texts include a majority of post-verbal enclitic sequences. Whereas *lo mi* itself is not exemplified, *'lmi*, *lim'*, *limi* and *la mi* are. Most frequently, however, the I and II clitics in such sequences are *ci* and *vi*, forms which are also used as locatives (further evidence of the same sort is cited in Melander, 1929). Perhaps the infiltration of such 'adverbials' into the pronoun system at this period led to confusion. By the mid-13th century the sequence ⌜*me ne*⌝ was usual (*ne* at that time still a I plural pronoun as well as a locative or a partitive pronoun). Later, in the 15th century and still today in non-standard usage, the *ne* of *gliene* functioned as a III ACC: it may be that the

existence of the ⌜*me ne*⌝ sequence encouraged the adoption of ⌜*me lo*⌝ (⌜*me*⌝ for ⌜*mi*⌝ in such combinations is variously explained as a re-analysis of *m'elo*, as a Bolognese borrowing, or as on the analogy of ⌜*me ne*⌝ where vowel dissimilation had operated). The evidence of the Decameron (1348-1353), in which ⌜*ne lo*⌝ is much more frequent than modern ⌜*lo ne*⌝, but where ⌜*lo mi*⌝ and ⌜*me lo*⌝ are about equally as frequent, may be of relevance: ambiguous *ne* could be in some contexts a I pronoun, and surface analogy favoured the acceptance of *me lo*.[10] Incidentally, many of the 14th century examples have forms *mel, sel, nel* etc, with the masculine III ACC apparently enclitic to, perhaps, a slightly stressed antecedent (cf the French 13th century *tel*).

The characteristic vocalism of the Tuscan I, II, REFL clitic (*mi, ti, si, ci, vi*) may have originally been a DAT marker (*mi* ← *MIHI*? compare also *gli, li*): phonologically regular development from *ME, TE, SE* would presuppose proclitic treatment at a period when the pronouns were probably enclitic. The case marker, if so it was, however, would quickly become redundant, especially when the DAT + ACC order was generalized: in the standard today *mi, ti* etc are used when alone or in combination with each other, *me, te* etc being used with III or *ne*. The shift from a morphological to a syntactic marking of case function accompanied a change from enclisis to proclisis and the replacement of 'simple' by 'special' clitics (cf *no/ne, vo/ve* replaced by *ci, vi* in Tuscan - in non-standard speech *ci* is also used as III DAT, in the standard it is also neuter as well as a demonstrative, and a replacement for *si* in *si si* combinations → (standard) *ci si*, (non-standard) *si ci*.

Although rigid ordering has not yet been successfully imposed on all Italian speakers, standard usage was more or less stable by the end of the 15th century (although ⌜*mi gli*⌝/⌜*gli mi*⌝ types of combination - unattested in the Castellani texts - were condemned only in the 20th century, and the (non-clitic) plural III *loro* was not in general use). The example set by Italian courtiers may then have influenced 16th century polite French usage, in that the ⌜*me le*⌝ order was preferred to ⌜*le me*⌝. The modern French 'soccer team' alignment was codified first by Oudin (1640): perhaps *le lui* and *y en* escaped Italian influence because the forms are less transparently related to Italian ones than *me, te, se*. Besides, we recall, the *le lui* sequence was never popular and the *y en* sequence occurs virtually only in the ⌜*il y en a*⌝ type of sentence in modern French, though Maupas' example was *Je m'y en retourne* (Vaugelas, Oudin and the Academy condemned *il en y a*).

To sum up, we conclude that the alleged mysterious pan-Romance drift towards rigid DAT + ACC clitic order is illusory. Pronoun sequences are rare enough for them to be ignored by most grammars and usage was variable, with preferences determined by euphony and analogy: it is probably the obsession with tidiness of early classical Romance grammarians that led to the formulation of strict rules. On the whole a LESS ACTIVE-LEAST ACTIVE order has been preferred, but the different languages have arrived at different solutions in sorting out the other functions to be signalled, possibly drawing on the

experience of other languages meanwhile, but also, again, influenced by phonetic factors and surface analogies. The activity of the grammarians from the 15th century on was of paramount importance: in order to make the vernaculars fit to rival Latin, precise rules had to be formulated, which were then, via social pressures and the education system, imposed on usage. Any argument - *bon usage*, logic, avoidance of ambiguity, Latin or Romance analogues - would serve to bolster a grammatical rule, which may appear 'crazy', especially when it is based on an incorrect analysis of usage. But a rule is a rule, and 'purity of language' is highly prized in the Romance world. Might it not be wrong-headed to try to fit such rules into abstract competence grammars, without taking account of the violations of the rules that occur in everyday performance?

What I have done is to take two different, but related, pan-Romance syntactic features and to show that they may derive from different kinds of 'collusion' between the languages - one via a spoken pidgin developed by Romance-speaking Crusaders, the other via a common yearning for the creation of tidy grammars on the classical Latin pattern. Other sources of collusion are, of course, direct interaction between the languages, and imitation of and borrowing from Latin. The collusion between the Romance languages may account for as many pan-Romance features as does a postulated Proto-Romance. In a sense, the languages have 'decreolized' during the course of history, focussing not only towards the model language, Latin, but also towards an 'ideal' grammar which owes a great deal to grammatical thought inspired by Latin.

FOOTNOTES

1. That the conjunctive subject and object pronouns are syntactically different from ordinary NPs and the disjunctive pronouns that substitute for them is argued at length by Kayne (1975) for French: he seems to me to be labouring the obvious. That they are 'special' in Zwicky's terms - in that they are also phonologically distinct from disjunctive pronouns - seems to be less obvious. Kayne suggests that they are allomorphic variants in French but Emonds appears to think that, except for the 'suppletion' *eux/leur* and the *le/lui*, *la/elle* lack of match, French object clitic and non-clitic pronouns can be phonologically derived from the same underlying forms.

2. Incidentally, this evidence seems not to square with Emonds' suggestion about the clitic node and its relationship to reflexives.

3. Emonds avoids reference to 'case' by claiming that the *lui/leur* forms in French replace *à* + NP. I use DAT and ACC for the forms rather than the functions.

4. Seuren (1976) tries to explain constraints of co-occurrence of

object clitics by assigning numerical loads and indices to them and suggesting that too 'heavy' clusters are avoided.

5. Melander (1929) suggested that (2) represented a pan-Romance order at which some languages arrived more quickly than others. Why some languages ever preferred (1) he was at a loss to know, though he did cite a suggestion that French placed the accusative nearest the verb. Hence verb enclitic sequences would favour (1) and verb proclitic (2). This is virtually the same as the solution proposed by Santangelo and Vennemann (1976).

6. How the modern English order in *I give him it* etc squares with Santangelo and Vennemann's (1976) suggestion that D is always closer to the verb than I is difficult to see.

7. C Leclere (1975) cites popular expressions such as:

Paul te m'a donné une de ces gifles
Paul te vous lui a donné une de ces gifles
Au Mont St Michel le mer te vous monte à une de ces vitesses

which violate constraints proposed by Perlmutter (1971). However, both Emonds and Seuren recognize that where the ethic dative is used, a different treatment of clitic clusters is required.

8. It is to be noted that where the III DAT is *hi*, a CV sequence is preferred.

9. Melander (1929) in citing evidence for the '*me lo*' sequence, nevertheless gives a number of counter-examples, often appealing to outside (eg Gallo-Roman) influence.

10. Melander (1929) rejects the suggestion that '*me lo*' influences '*me ne*' sequences, on the grounds that *ne lo* appears later in texts, but this would not apply to the relative chronology of *ne lo* and *me lo*.

REFERENCES

Academia R S Romînia
1963 Gramatica Limbii Romîne Vol I, Bucharest: Ed Acad.

Badía Margarit, A
1947 Los complementos pronomino-adverbiales derivados de 'ibi' e 'inde' en la Península Ibérica, Madrid: CSIC.

1951 Gramática histórica catalana, Barcelona: Noguer.

Bredemeier, J
1976 Strukturbeschränkungen im Rumänischen. Studien zur prä- und postverbalen Pronomina, Tübingen: Niemeyer.

Brunot, F
1905-53 Histoire de la langue française Vols 4-6, Paris: Colin.

Castellani, A
1952 Nuovi testi fiorentini del dugento Vol I, Florence: Sansoni.

Close, E
1974 The Development of Modern Rumanian. Linguistic Theory and Practice in Muntenia 1821-1838, Oxford: University Press.

Costa, R
1975 'A Functional Solution for Illogical Reflexives in Italian', in CLS Parasession on Functionalism: 112-25.

Diez, F
1876 Grammatik der romanischen Sprachen, Bonn: Weber.

Emonds, J
1975 'A transformational analysis of French clitics without positive output constraints', Linguistic Analysis 1.1: 3-24.

1976 A Transformational Approach to English Syntax. Root, Structure-Preserving and Local Transformations, New York: Academic Press.

Figueiredo, C de
1928 O problema de colocação de pronomes, Lisboa: Teixeira.

Fries, C C
1940 'On the Development of the Structural Use of Word-Order in Modern English', Language 16: 199-208.

Galet, Y
1971 L'évolution de l'ordre des mots dans la phrase française de 1600 à 1700. La place du pronom personnel complément d'un infinitif régime, Paris: PUF.

García, E C
1975 The Role of Theory in Linguistic Analysis: The Spanish Pronoun System, Amsterdam: North Holland.

Green, J N
1976 'How free is word order in Spanish?', in M B Harris (ed.) Romance Syntax: Synchronic and Diachronic Perspectives, Salford: U.P., 7-32.

Gregor, D B
1975 Friulan: Language and Literature, New York: Oleander.

Iliescu, M
1972 Le frioulan à partir des dialectes parlés en Roumanie, The Hague: Mouton.

Kahane, H and R Kahane
1976 'Lingua franca: the story of a term', RomPh 30: 25-41.

Kayne, R S
1975 French Syntax: The Transformational Cycle, Cambridge, Mass: MIT Press.

Leclere, C
1975 'Datif syntaxique et datif éthique', Rapport de Recherches, Lab. d'Aut. doc. et ling. 5: 27-59.

Le Page, R B
1975 '"Projection, focussing and diffusion" or, steps towards a sociolinguistic theory of a language'. Paper delivered at the Conference on the methodology of sociolinguistic surveys, Montreal. Washington DC: Center for Applied Linguistics. (Reprinted in this volume, 9-31).

Lerch, E
1940 'Proklise oder Enklise der altfranzösischen Objektspronomina? (Monatische und pluralistische Sprachanschauung)', ZRPh 60: 417-59.

Mattoso Câmara, J
1972 The Portuguese Language, Chicago: U.P.

Melander, J
1929 'L'origine de l'italien *me ne, me lo, te la* etc', Studia Neophilologica 2: 169-203.

Meyer-Lübke, W
1897 'Zur Stellung der tonlosen Objektspronomina', ZRPh 21: 313-34.

Morin, J-Y
1975 'Old French clitics and the Extended Standard Theory', CLS 11: 390-400.

Napoli, D J
1976 The two si's of Italian: an analysis of reflexive, inchoative and indefinite subject sentences in Modern Standard Italian, Bloomington: Indiana University Linguistics Club.

Niculescu, A
1974 'Premesse sul problema dei rapporti cultural-linguistici italo-romeni', in Actele celui de-al XII-lea Congres Internaţional de Lingvistică şi Filologie Romanică, Bucharest: Editura Academiei, 2: 893-905.

Olsen, H
1928 Etude sur la syntaxe des pronoms personnels et réfléchis en roumain, Copenhagen: Høst.

Olszyna-Marzys, Z
1964 Les pronoms dans les patois du Valais central. Etude syntaxique, Berne: Francke.

Par, A
1923 Sintaxi catalana segons los escrits en prosa de Bernat Metge (1398), Halle: Niemeyer.

Perlmutter, P M
1970 'Surface structure constraints in syntax', Linguistic Inquiry 1: 187-255.

1971 Deep and Surface Constraints in Syntax, New York: Holt, Rinehart and Winston.

Pinchon, J
1972 Les pronoms *en* et *y*. Problèmes généraux de la représentation pronominale, Geneva: Droz.

Pittau, M
1972 Grammatica del Sardo-Nuorese. Il più conservativo dei parlari neolatini, Bologna: Pàtron.

Poalelungi, A G
1973 L'influence du français sur le roumain (vocabulaire et syntaxe), Paris: Belles Lettres.

Pope, M K
1934 From Latin to Modern French with especial consideration of Anglo-Norman, Manchester: University Press.

Posner, R
1974 'The Ordering of Historical Phonological Rules in Romance', TPS: 98-127.

Prader Schucany, S
1970 Romanisch Bünden als selbständige Sprachlandschaft, Berne: Francke.

Rohlfs, G
1949 Historische Grammatik der Italienischen Sprache und ihrer Mundarten Vol II, Berne: Francke.

Ronjat, J
1930-41 Grammaire Istorique des Parlers Provençaux Modernes, Montpellier: Soc des Langues Rom.

Rosetti, A
1966 Istoria Limbii Române Vol VI, Bucharest: Ed Acad.

Seuren, P A M
1976 'Clitic Pronoun Clusters', Italian Linguistics 2: 7-36.

Santangelo, A and T Vennemann
1976 'Italian Unstressed Pronouns and Universal Syntax', Italian Linguistics 2: 37-46.

Solà, J
1973 Estudis de Sintaxi Catalana Vol II, Barcelona: Ed 62.

Sorrento, L
1950 Sintassi Romanza. Richerche e prospettive, Varese-Milano: Cisalpino.

Straka, G
1965 'Naissance et disparition des consonnes palatales dans l'évolution du latin au français', TLLS 3: 118-51.

Suñer, M
1974 'Where does impersonal *se* come from?', in Campbell *et al* (ed.) Linguistic Studies in Romance Languages, Georgetown: University Press. 146-57.

Szabo, R K
1974 'Deep and surface order of Spanish clitics', ibid: 139- 45.

Talmy, G
1971 'Historical syntax and synchronic morphology: an archeologist's field trip', CLS 7: 394-415.

Theban, L and M Theban
1971 'La syntaxe des langues romanes et l'universalité des structures profondes', Bull de la Soc Roum de Ling Rom 8: 23-36.

Togeby, L
1974 Précis historique de grammaire française, Copenhagen: Akademisk Forlag.

Vasquez Cuesta, P and M A Mendes da Luz
1961 Gramática portuguesa, Madrid: Credos.

Vaugelas, C F de (ed. J Streicher)
1934-36 Remarques ... et commentaires, Paris: Droz.

Wagner, M L
1938 'Flessione nominale e verbale del sardo antico e moderno', Italia Dialettale 14: 113-26.

Wanner, D
1974 'The evolution of Romance clitic order', in Cambell *et al* (ed.) Linguistic Studies in Romance Languages, Georgetown: University Press, 158-77.

Wackernagel, J
1892 'Über ein Gesetz der indogermanischen Wortstellung', Indogermanische Forschungen 1: 333-435.

Zwicky, A
1977 On Clitics, Bloomington: Indiana University Linguistics Club.

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ON THE INTERPRETATION OF THE COMPLEMENT NOUN PHRASES OF ILLOCUTIONARY/PERLOCUTIONARY AND TRANSITIVE VERB-OF-MOTION PREDICATES

Two distinct interpretations of the noun phrase objects in the prepositional phrase complements of certain verbs are possible in English. The locative noun phrase in the locative prepositional phrase complement of a transitive verb of motion is normally interpreted as having a concrete referent, as in (1):

(1) The bully pushed his victim's face into the mud.

If however the identical verb is used to report an illocutionary or perlocutionary act, the interpretation (effect) is different, as in:

(2) The hostess pushed her guest into another trifle.

This distinction can be observed also in the noun phrases of the prepositional phrase complements of many other verbs that can be used to report either causative or illocutionary/perlocutionary acts, as (3) and (4) show:

(3) The air traffic controller talked the pilot (in)to a safe landing.

(4) The father talked his son out of a new car.
... into a haircut.

The distinction can be explained by positing a special kind of existential predicate, of which the complement noun phrases in sentences like (2) and (4) are the objects; it is the abstract proposition formed by the noun phrase and this predicate that is the illocutionary goal or object in such sentences, rather than the noun phrases themselves. The Addressee (illocutionary target) of the governing verb controls (ie is the 'subject' or 'agent' of) the special predicate; the pragmatics of language use are assumed to determine the precise interpretation of this control in specific situations.

Most, if not all, of what I have to say, is already embodied in the foregoing; the rest of the discussion will therefore be devoted to developing the details implicity if not already explicitly stated.

In sentence (1),

(1) The bully pushed his victim's face into the mud.

we have the following elements, taking them in linear order, going from left to right: first, the grammatical subject of the main verb. It also happens to be the logical subject, so-called, as well as the active agent, the source of the active motive force required to perform or carry out the action named by the main verb. The second element is the main verb, *push*. This is a transitive verb of motion, that is, its elementary semantic analysis or composition is CAUSE-GO. Next comes the target of the action named by the main verb, the noun phrase, *the victim's face*. This NP is not only the target of the action of *causing*; it is also the subject of the act of *going*, a semantic fact that I will leave undeveloped syntactically in this discussion. Finally, we have the transitive or directional locative goal or destination of the target of being caused to go, the noun-phrase, the *mud*, which is the object of the complement prepositional phrase. The fact that the target of the act of causing to go actually ends up in the location that constitutes the goal is represented, not entirely redundantly, by the preposition *into*. Substitution of other prepositions would give different spatial configurations of the act, the target, and the goal. Consider the effect, for example, of replacing *into* with *towards*, *up against*, *away from*, *out of* etc. (See P-Marker 1)

All this information is brought together in P-Marker 1. In order to show that *push* represents, not just the elementary semantics of the class of transitive verbs of motion, that is CAUSE-GO, but is also the expression of the manner, mode, means, or instrument of motion, we represent it separately as the main verb of a Manner or Instrumental sentence or proposition whose subject must of course be identical to the higher grammatical subject of the main PRO-Verb complex, CAUSE-GO.

A simple rule of raising will lift the verb of this Manner or Instrumental sentence or proposition into the immediately dominating higher PRO-Verb node, replacing it, or, perhaps better, being conflated with it. (This process is very productive in English, although the superficial trivality of the present example might not suggest very much about its importance.)

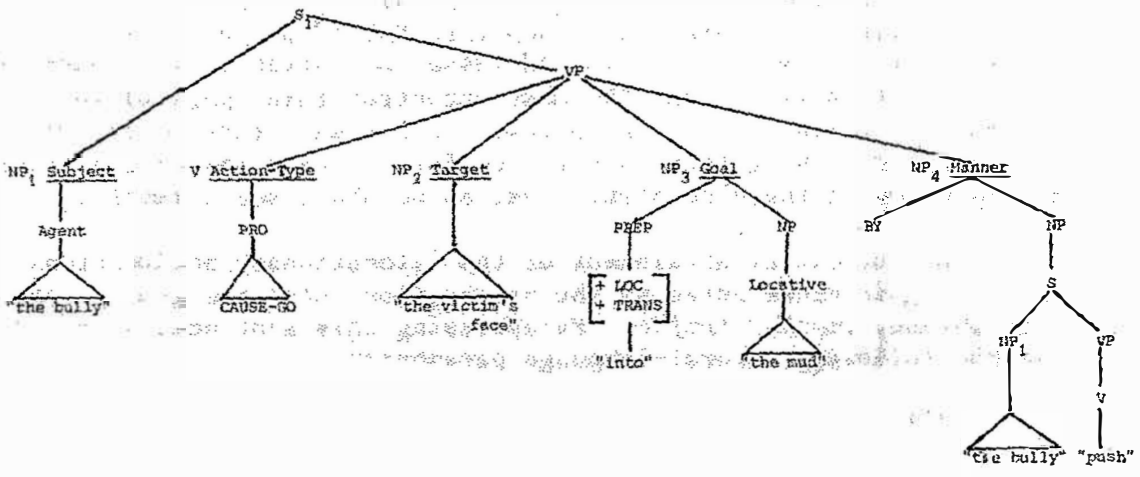
Now we apply the same simple-minded approach to sentence (2):

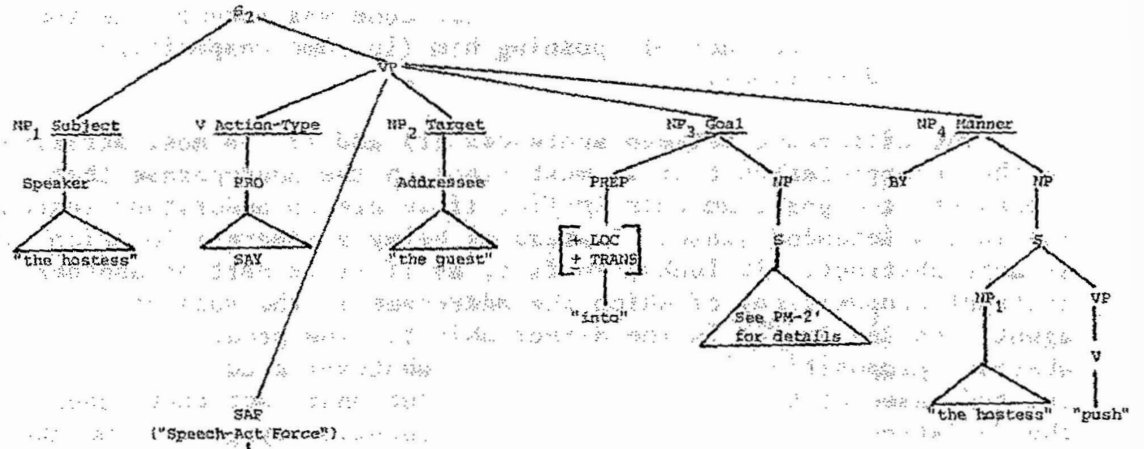
(2) The hostess pushed the guest into another trifle.

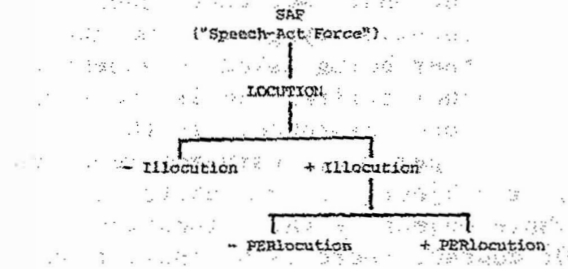
(See P-Marker 2)

Going through the sentence in the same way, from left to right as before, we note some major differences between (1) and (2). First, the grammatical subject, the noun-phrase, *the hostess*, is the speaker, or rather, the reported illocutionary source in a report of a successful perlocutionary act reported by the sentence, and she effects her perlocutionary goal by something that she says, or, more generally or more exactly, something that she communicates, to the Addressee. The main verb *push* represents here, not only some metaphorical act of pushing, as in (1), but also an underlying locutionary act with illocutionary force or intent, and perlocutionary effect. Since the elementary semantic prime or higher PRO-Verb SAY

P-MARKER 1

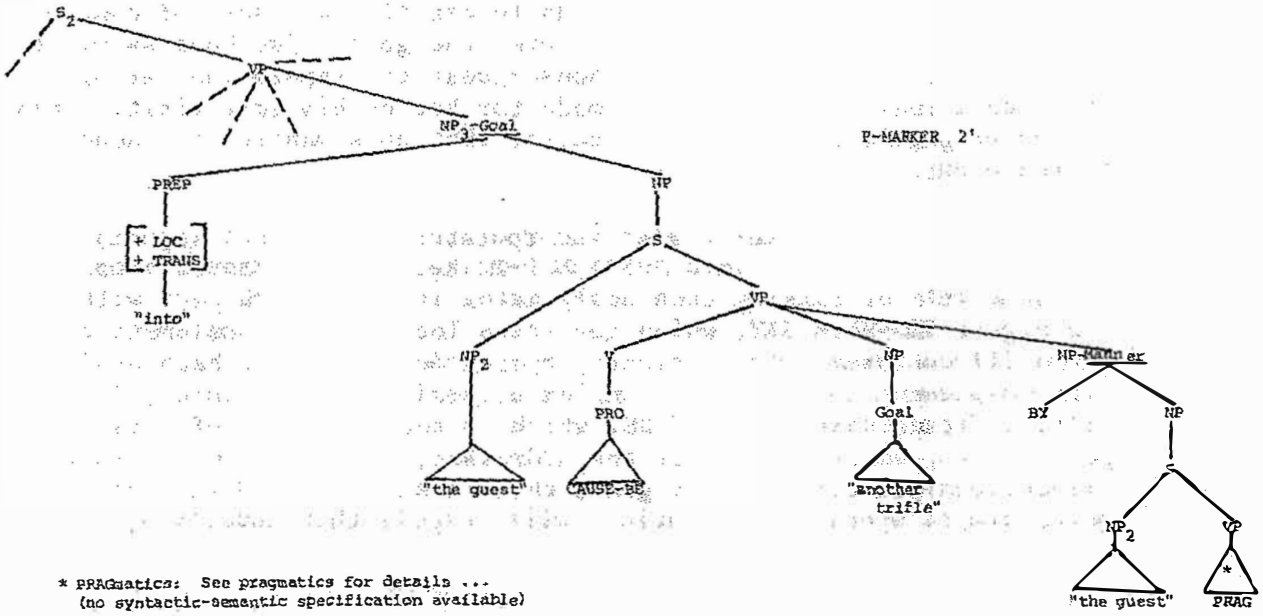






P-MARKER 2

P-MARKER 2'



* PRAGmatics: See pragmatics for details ...
(no syntactic-semantic specification available)

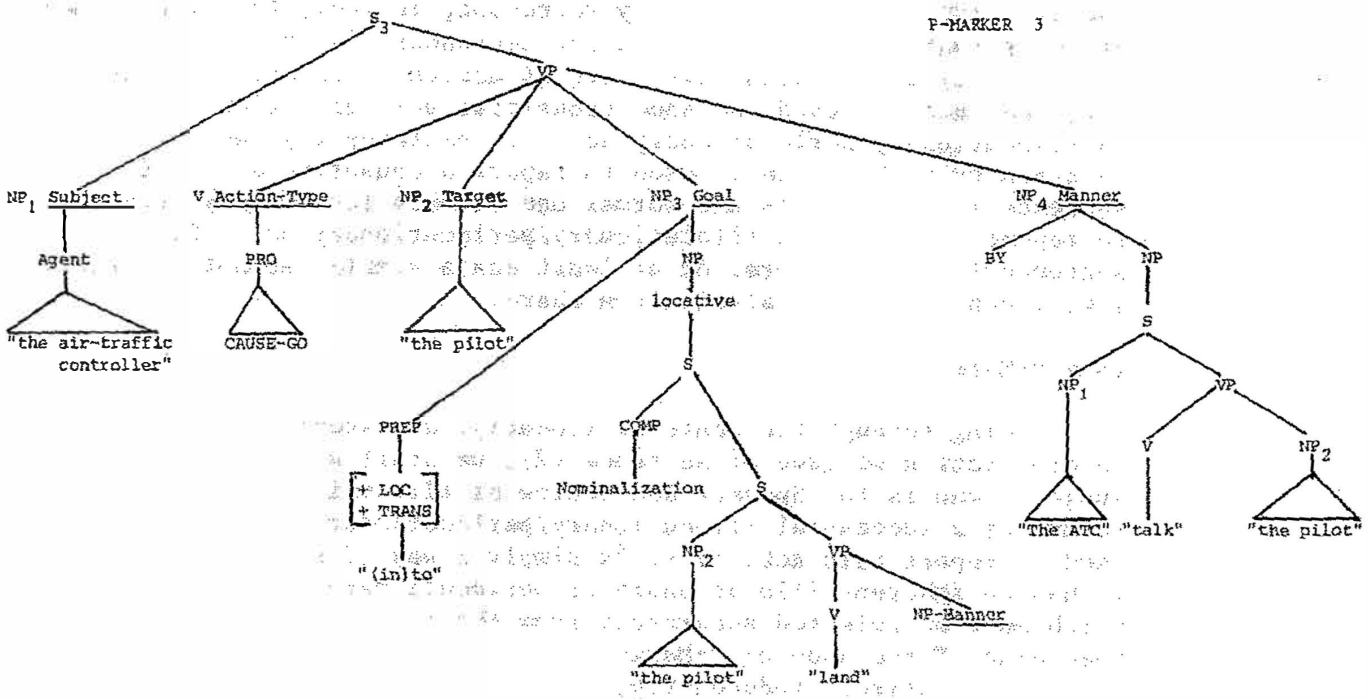
does not in or of itself contain or convey any speech act force except that of locution, it is necessary to represent these semantic primes separately as a separate SAF ('Speech-Act Force') constituent in P-Marker 2. The Addressee or target is the noun-phrase *the guest*; this is perhaps the most uncontroversial part of the whole schema; the semantics of the Addressee are always that of direct reference, something that is not true for at least one of the other noun-phrases in these constructions, as we shall see directly.

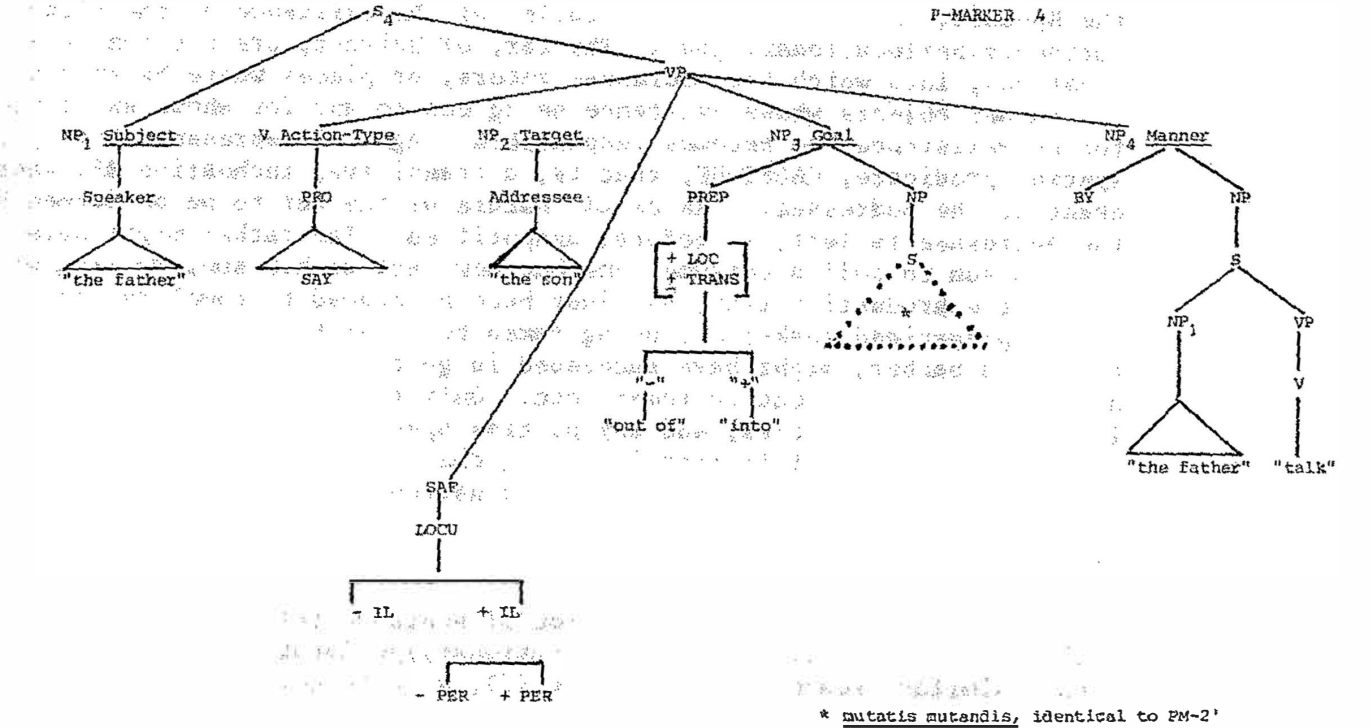
The successful attainment of the illocutionary/perlocutionary goal is again represented by the preposition *into*; the goal is the noun-phrase, *another trifle*. Paraphrasing this sentence, we might use the following natural-language paraphrase or version of P-Marker 2:

(2') The hostess successfully persuaded the guest to accept, have, or agree to, another helping of trifle, by saying or communicating something (unspecified) to him; and the manner in which this was done was rather like the physical act of pushing him (in some unspecified direction).

The difference between sentences (1) and (2) is most striking in the interpretation that we must place on the noun-phrase that represents the goal, *another trifle*, if we are to understand sentence (2) in its intended sense. Instead of being a concrete location, it is more abstract. It looks, in fact, as if it is part of another abstract proposition, of which the Addressee is the subject or agent, and *the trifle* is the direct object. The predicate of this abstract proposition is the action - of whatever kind - performed by the Addressee of the illocutionary/perlocutionary act that leads to the 'existence' of the illocutionary/perlocutionary goal. The guest does not end up in the trifle, but is rather being asked to assent to the proposition that there should be another trifle. He is the giver of life or death, so to speak, to its future existence. In this sense, the Addressee of the illocutionary/perlocutionary act reported in this sentence is the *controller*, or subject, of some abstract action of calling into existence, whose object is the illocutionary/perlocutionary goal, *the trifle*. Of course, there is no indication in the semantics of this sentence as to exactly what sort of control it is that the Addressee exercises over the goal. The hostess might just as well have been asking a house-guest to prepare another of those delicious trifles that he made for her on his last visit. This is the pragmatic aspect of the control that no semantics can hope to represent.

Note that P-Marker 2 also incorporates the subject (agent) and predicate (the lexical verb *push*) of P-Marker 1 as its Manner proposition. The same rule of raising then amalgamates its main verb *push* with the higher PRO-Verb SAY, which governs a locutionary complement to which illocutionary/perlocutionary Speech-Act Force has been assigned. This complement is in fact a complex proposition, containing, if you will, a 'transitive' form of BE, which is the main verb of this proposition, whose subject is the Addressee, and whose object is the illocutionary/perlocutionary goal; this is shown in P-Marker 2'. Quite simple syntactic operations will respell this underlying





* mutatis mutandis, identical to PM-2'

P-Marker, when the relevant bits are filled with suitable lexical items, as our sentence (2).

(See P-Marker 2')

A treatment similar, if not identical, to that offered for sentences (1) and (2) immediately suggests itself for sentences (3) and (4), *mutatis mutandis*. They represent, in fact, the mirror image of the situation I have outlined for sentences (1) and (2). Sentence (1) used a transitive verb of motion to report a causative act; sentence (2) used the same transitive verb of motion to report an illocutionary/perlocutionary act. In sentence (3), we have a locutionary verb, which is used to report a causative act, and sentence (4) represents the normal use of this locutionary verb, *talk*, to report a successful illocutionary/perlocutionary act. Since sentence (4) is the norm, or at least has a simpler semantic structure, I will continue the analysis from there.

(See P-Marker 4)

Going through the sentence linearly, and keeping in mind the interpretation we gave of sentence (2), we start with the grammatical subject, who is the Speaker and source of illocutionary force in a report of a successful illocutionary/perlocutionary act. The verb used to report this act, *talk*, is simply a way of saying something; it has no inherent illocutionary or perlocutionary force of its own, which must be selected separately from the choices available in the Speech-Act Force node of P-Marker 4. Successful perlocution is shown, not entirely redundantly, by the preposition, *into*, or, as the case may be, by its negative opposite, *out of*, which introduces the noun-phrase that is the name of the illocutionary/perlocutionary goal. The Addressee, that is, the illocutionary target, plays a dual role, as I showed in the case of sentence (2): Not only is he the intended recipient, ie illocutionary target, of the message issued by the Speaker; he is also the controller of the existence of the illocutionary/perlocutionary goal. The car, or haircut, are not concrete locations, into which the Addressee enters, or places where he ends up, but rather objects whose existence he agrees to and for whose existence (or non-existence) he becomes responsible. Again I represent this by a special predicate, CAUSE-BE, that is, a transitive, inchoative BE, whose agent is the Addressee. The exact nature of the act to be performed by the Addressee is left, as before, unspecified. The father might have got his son to sell a consumer another new car; or his son, working with him on the production line, has just been persuaded to complete another of his prescribed tasks; *new* being taken to refer to another; just as the father, a barber, might have succeeded in getting his son to do another haircut on an impatient customer etc. Only the pragmatics of real life limits the possibilities, and any precise specification is due to concrete, circumstantial factors involving the actual use of the sentence; it is not part of the semantics, in the narrow sense.

(See P-Marker 3)

Contrasting this interpretation of sentence (4), its main verb *talk*, and the semantics of its illocutionary/perlocutionary goal, with a similar treatment of sentence (3), we note certain crucial

or criterial differences. (See P-Marker 3.) The grammatical subject of (3), although a noun-phrase as in (4), is the Agent or cause, of the whole action named by the predicate, rather than merely the Speaker. The Verb *talk* is being used to report, not a locutionary or illocutionary/perlocutionary act as such, but rather a causative action, one of bringing something about. The something is, here, that the pilot goes into a new situation, one in which there is a safe landing. (Although landing is the nominalization of the verb *to land*, or *to make a landing*, it could just as well have been a concrete locative, as in the complex transitive locative goal, *down onto the runway*.) What is brought about is that the pilot is *in* a safe landing (or down on the runway). Disregarding *landing* as the nominalization of the verb *land*, we require only a simple metaphorical extension of our notion *locative* to preserve the similarities and identities between sentence (1) and sentence (3).

The verb *talk* describes the manner, mode or means whereby the ATC brings it about that the pilot goes into the safe landing. The ATC is the *causer* or controller of the existence of the safe landing, even though the pilot is in some other sense the subject of the verb that underlies the nominalization *landing*. In any case, since *talk* is, originally, not a causative but a locutionary verb, it originates in a manner proposition that looks much more like P-Marker 4 than the relative simple or axiomatic manner proposition of, say, P-Marker 1; that is, it brings here more semantic structure with it.

Of course, following on from everything that has been said, we see in fact that sentence (3) is ambiguous. Giving it the interpretation that we gave to sentence (4), and restricting the semantics of *talk* to mere locution, sentence (3) could mean that the pilot was persuaded to agree to make a safe landing, or to teach this skill to his student pilot. The point is that there is an important difference between two apparently identical surface structure forms. A sequence such as (5):

(5) $(\text{NP } (\text{V NP } (\text{into NP})_{\text{PrepPh}})_{\text{VP}})_{\text{S}}$

cannot be interpreted without a semantics that differentiates between different Action-types, and between at least two different kinds of noun-phrases in the 'locative' complements of whatever verbs are represented at the V-node, and among various Subject-types.

This is not just some accidental piece of natural language vagueness, as has been claimed or suggested, but can be supported by further examples. Consider, first, sentence (6):

> (6) The 40 per cent rule, whereby that number of the total [Scottish or Welsh] electorate must vote YES to the [Devolution] Act if it is to be put into effect, sometimes appears to be filling as many column-inches in the Scotsman and Glasgow Herald as the Act itself. Proposed by a Scot who sits for a London constituency, George Cunningham, it was seized on by the English backbenchers when the Scotland and Wales Bills were being debated last year, and the Government was sandbagged into it.
> (Paul Ferris, 'Reality behind the Scots trumpets', The Observer 25.2.79:11)

The verb *sandbagged* is semantically and pragmatically intended to refer, metaphorically, of course, to doing something to someone with sandbags, presumably hitting them about the ears or some such appropriately sensitive place. One could talk of 'sandbagging the rock into position', ie 'causing the rock to go into position by sandbagging it'; the P-Marker (not given here) that would represent the semantics of that sentence is identical to P-Marker 1, with the exception that the manner proposition would have to contain a much fuller representation of the semantics of *sandbag* than it needs to do for the semantics of *push*, since *push* is already the name of a manner or means of moving something, ie is already a transitive verb of motion, while *sandbag* is not.

Fitting the new, or more complex version of P-Marker 1 with *sandbag* into the appropriate slot in P-Marker 2, that is, in the manner proposition or node, our simple operation of raising will add the lexical verb *sandbag*, and its semantics to the semantics of the higher PRO-Verb, SAY, which lends the required illocutionary/perlocutionary reading to *sandbag*. At the same time, we must interpret the noun-phrase *it*, referring to the *40 per cent rule*, as being the object of the abstract transitive form of the predicate BE that we have seen in sentences (2) and (4) above. That is, it is the object noun-phrase in an axiomatized sort of abstract proposition, whose subject is the government; its main verb is an abstract reference to this subject's role in bringing about the existence of this object noun-phrase, without mentioning specifically how this comes about. (To do this, one must of course restore the passive 'The Government was sandbagged into it' to its normal active form, 'NP sandbagged the Government into it'.)

Consider next sentence (7):

(7) What cowardice! Some foolishness had tempted him to a rendezvous which, even if Mrs Willyams had been Cleopatra, would have been by that degree more reckless.

(Maurice Edelman, Disraeli Rising, p 288 (pb))

The correct interpretation here of *rendezvous* depends as before on the correct interpretation of *tempt*. Interpreting *tempt* as being an illocutionary verb, or one that reports an illocutionary (sometimes perlocutionary) act, we obtain an interpretation of *rendezvous* similar to that which we ascribe to the noun-phrase complements or goals of sentences (2) and (4). Interpreting *tempt* as a causative, on the other hand, that is, virtually as a transitive verb of motion, say like *bring* or *take*, gives us the concrete, locative interpretation of *rendezvous*, that is, as the place, not as the agreement or arrangement. As Disraeli is at the actual place of rendezvous, one is tempted to assign to *tempt* the transitive verb of motion reading, and to *rendezvous*, the concrete locative reading, especially as the preposition *to*, which is the unmarked member of the class of prepositions *towards, to, into, up to*, often has the implication, 'actual arrival at the locative or illocutionary goal', when it is used with a locative complement that has a concrete meaning, such as a place or location, or, with illocutionary verbs, nominalizations as goals, as we saw in (3). But the other reading - with

tempt as merely illocutionary - is just as possible. That is, with *tempt* being given its normal illocutionary reading, and *rendezvous* the abstract, existential reading (something like *agree to* in this case) this sentence is just as compatible with the context on semantic and pragmatic grounds as the other reading. The ambiguity remains, however.

Examples such as these can be multiplied endlessly. (See Appendix.) What I am saying, if true, is that speakers of English (and some other, but by no means all, languages) make quite subtle semantic distinctions among various types of verbs or their action types, as part of their daily linguistic activity, and the correct use of these action types requires the grammar to possess fairly detailed semantic representations of individual sentence or surface-structure types and an associated mechanism whereby verbs that seem, from the ordinary meaning point of view, to belong to one action type, can be transferred to another action type, as well as a special mechanism for interpreting the semantics of their complement noun phrases.

NOTE

This paper was given at the Spring Meeting of the Linguistics Association of Great Britain at Hull University in March 1979, and is reproduced here without alteration, except that the final paragraphs have been omitted as being an irrelevant distraction. It is really more like a progress report, excerpted from a much larger and more comprehensive study of certain natural classes of verbs in English (here called 'action-types'). By studying the semantic and syntactic properties of these verbs, and the creative syntactic processes that give rise to the examples cited and discussed here, this investigation makes a contribution to our understanding, not only of the inherent formal variability in language, but also of the linguistic basis of sociolinguistically conditioned variability. I am therefore pleased to offer it to Bob Le Page, as having been directly stimulated by my desire to confront the originally monolithic concept of a generative grammar with the facts of the indeterminacy in the form of language. In fact, a quite simple adjustment in our ideas about how a generative grammar must work, and our willingness to accept that semantic considerations can be quite as important as syntactic ones in determining the operation of syntactic and interpretive rules, allow the concept of generative grammar to accommodate the facts of linguistic variability without difficulty.

For the many years of thought and observation that led me to some of the conclusions or proposals put forward here, I am indebted in the first place to my former fellow graduate student and colleague, Mary Sleator Temperley, who first directed my attention to the linguistic riches that can be mined from any local or national newspaper. Second, I wish to thank Tom Wachtel, Tony Sudbery and Margaret Maclure for making some valuable suggestions which I have incorporated here without properly crediting them. Finally, to Geoff Pullum I owe the suggestion (which, as can be seen, I reject) that the perceived distinction in how the noun phrase objects in the

examples cited are to be interpreted 'is probably pragmatic'. This chance remark induced me to reflect on the distinction between 'pragmatics' and 'semantics', which I think can be viewed in the following way: Pragmatics deals with the use made of utterance *tokens* in concrete situations to effect conversational goals, all of which is a very complex activity ('performance') that takes place within a complex network of social and personal relationships and cultural institutions. Semantics deals with those invariant meaning properties of the axiomatic elements and their structural relationships that form the basis of the input to the syntactic component which ultimately determines the resulting form ('surface structure') of sentences. Thus semantics includes all those propositions that are entailed by the structure and possibilities of interpretation of a given sentence or sentence type, but all independently of the use to which that sentence is put in performance. That is, I am assuming that the meaning properties of the example sentences discussed here can be studied abstractly without reference to the concrete situation of utterance. All the latter contributes is the information required to determine which of the several possible interpretations is the intended one. This is the familiar and almost universally accepted definition of ambiguity. Semantics deals with ambiguity; pragmatics deals with vagueness and indeterminacy, resolving them by referring to situational clues for possible intentions.

The conversion of pragmatically determined aspects of sentence use and interpretation into elements of semantic invariance must be one of the most important processes whereby language comes about in children. Language permitting, semantic invariants can be converted into structural properties of sentences. In the representations of semantic structure (the P-Markers), it may have escaped the notice of the reader that the *same* representation can be used for pragmatic, semantic and syntactic structure. The impression of 'generative semantics' which is suggested by these P-Markers is purely accidental. In fact, my proposals owe virtually nothing to that scholastic exercise, but arise out of the need to represent certain semantic and syntactic properties of the examples concerned. They are empirically induced, so to speak, and have been invented *ad hoc* because of my desire and intention to explain the data in particular ways. It is only in considering the productivity of the semantic and syntactic mechanisms themselves that one achieves the highest level of 'observational adequacy'. Data from other languages, as well as from child language, suggest that the mechanisms also have 'descriptive adequacy', that is, are psychologically 'real', but the evidence for this claim will have to wait for presentation elsewhere.

Finally, readers might check for themselves that the distinctions posited here in how to interpret noun phrases (and certain types of verbs) are semantic and syntactic in nature, and not pragmatic, by considering how language-specific they are. Slavic languages, such as Russian, Czech and Polish, with their systems of verbal aspect, and a highly developed set of preposition-based verb prefixes, are able to make quite subtle (by English standards) semantic distinctions quite overtly. Given the definition of pragmatics offered above (which might not be what Geoff Pullum had in mind), and the statement that the distinctions are 'probably pragmatic', then any

language could have such distinctions for arbitrary utterance tokens. That this is not the case is proof that the distinctions, whatever they are, are at least not pragmatic.

APPENDIX

The following two further examples illustrate not only the productivity of the processes outlined in this paper, but also the difficulties (only hinted at) in reaching a decision, in any individual case, on the 'correct' interpretation of the semantics of the distinctive elements of the construction. In example (a), for instance, what role is to be assigned to the grammatical subject, *the Dean*; is *urge* a transitive verb of motion (CAUSE-GO); is *the bicycle* the Addressee of an illocutionary act; is *farther* a directional locative complement, or an illocutionary goal, and so forth? In example (b), what are the semantics of *trouble* and *a penalty*? The chief interest of the passage is that *talk* must be assigned a semantic interpretation quite different from its normal locutionary use, it is more 'causative' in nature, and could be thought of as replacing an expected *get*.

(a) Meanwhile, the Dean, his bony legs pumping innocently, was urging his bicycle farther and farther into the salubrious countryside north of Oxford.

(Wilfred Sheed, A Middle Class Education, London: Sphere Books, 1967, p 20)

(b) It has always been possible to buy yourself out of trouble: nice to be able to afford the very best counsel to talk you out of a penalty. Take speed limits. ...

('How super is the Snooper? - MOTORING: Roy Harry reports on a new device for giving advance warning of police radar traps', The Guardian 1.10.79: 11)

Finally, in example (c) below, the verb *needle* has several potential readings (hence the ambiguity that is the source of the humour), and *it* - referring to *marriage* - is either the 'concrete' directional locative goal, or the (more indirect) illocutionary goal like *trifle* in (2).

(c) When I met my husband, he was a patient on the ward where I was an Air Force nurse. I had gone into his room to administer two shots in preparation for neurological diagnostic tests. Now, whenever someone asks my husband how he managed to trap and marry an officer, and a nurse at that, he just smiles and says, 'She needled me into it!' - Constance A Groff (Lanckland AFB, Texas).

(Reader's Digest, May 1979, 'Humor in Uniform', p 31)

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NETWORKS AND SOCIOLINGUISTIC VARIATION IN AN AFRICAN URBAN SETTING[1]

The following discussion is rooted in the conviction that people, in their everyday encounters, are largely dependent on others rather than on an abstract society, and that their behaviour is not entirely determined by culture, rules and belief systems. The notion of man 'as an interacting social being capable of manipulating others as well as being manipulated by them' (Boissevain's preface to Boissevain and Mitchell, 1973) is at the heart of the network concept, currently being developed and refined within the field of social anthropology, and already made use of in sociolinguistic work, for example by Gumperz (1971 with J-P Blom, 1976a, 1976b), Labov (1973a) and Milroy and Margrain (1978).

In this study the method of eliciting linguistic data was based on the notion of social network. One reason for taking such an approach is suggested by Gumperz (1976a):

> The sampling procedures of surveys ... assume that community or group boundaries exist, that they are generally recognized, and can easily be determined. In cases of language shift, where alterations in verbal behaviour go hand in hand with radical alterations in values and group identification, such assumptions of stability clearly don't hold. Here, notions of group boundaries and ethnic identity and standards of evaluation vary with context ...

However, even when a fairly clearly defined (on sociocultural criteria) group or community is the subject of attention the network concept is valuable (eg Gumperz and Blom, 1971) and in the present discussion I will follow Mitchell (1973) in regarding networks and corporate groups as social phenomena at different levels of abstraction and also accept his suggestion that 'networks of relationships are the starting-point in the analysis of group behaviour ... they exist as analytical constructs which the observer erects partly by taking the participants' perceptions into account and by fitting together observations not available to the participants themselves.'

The concept 'social network', it must be emphasized, does not in itself constitute a theory. Kapferer (1973) points out: 'A concept is one step beyond a description in the sense that it suggests a specified set of rules for organizing human experience .... Network and network density when applied to sociological data do not explain anything in themselves. An explanation is provided by a theory ....' Kapferer's study and that of van Velzen (1973) both apply exchange theory to network analysis. In the following discussion I shall try to indicate how a model closely related to exchange theory, ie the accommodation model, together with Le Page's (1968) 'hypothesis and

four riders', may be related to partial networks in order to provide a social explanation of linguistic variation and to help assess the likelihood of this variation constituting potential linguistic change.

Giles and Powesland (1975) give an account of the theory of accommodation and its applicability to linguistic code-choice and code-switching. Briefly:

> The essence of the theory of accommodation lies in the social psychological research on similarity-attraction .... This work suggests that an individual can induce another to evaluate him more favourably by reducing dissimilarities between them. The process of speech accommodation operates on this principle and as such may be a reflection of an individual's desire for social approval. In exchange theory terms (Homans 1961), it seems likely that the accommodative act may involve certain costs for the speaker, in terms of identity-change and expended effort, and so such behaviour may be initiated only if potential rewards are available. If one can accept the notion that people find social approval from others rewarding, it would not seem unreasonable to suppose that there may be a general set to accommodate to others in most social situations.

This quotation gives us the general orientation of the theory only. Elaborations and attendant concepts (as well as limitations) are discussed in Giles and Powesland, 1975 ; the concomitant notions of symmetrical and asymmetrical convergence and divergence in dialogues are of particular relevance to the present discussion. The stance of the statement quoted above is strikingly similar to that of Le Page's 1968 hypothesis:

> The individual creates his systems of verbal behaviour so as to resemble those common to the group or groups with which he wishes from time to time to be identified to the extent that:
>
> (a) he is able to identify those groups,
> (b) his motives are sufficiently clear-cut and powerful,
> (c) his *opportunities* for learning are adequate,
> (d) his ability to learn - that is, to change his habits where necessary, is unimpaired.

The relevance of this hypothesis and that of accommodation theory to my data will be discussed in due course.

The subjects who are to be the focus of the discussion are members of the Swahili-speaking Afro-Arab Muslim community of Mombasa, Kenya's major port. They were all, at the time the linguistic data were collected, resident in the densely-populated Old Town district[2] of Mombasa Island, and all of them had been born in Mombasa. The island forms part of the municipality of Mombasa and is joined to the mainland by two bridges (one of which carries the road and railway to Nairobi), a causeway and two ferries. Ever since missionaries and linguists first directed their attention to the language of the

Muslim communities of the East African coast in the middle of the last century, dialectal differences (phonological and morphological and, to a small extent, lexical) have been noted between the speech of Mombasa and Zanzibar and indeed other coastal settlements as far north as the southern end of what is now the Somali Republic.

The standard form of Swahili, which has achieved national language status in Tanzania and Kenya, is an artificial construct based on the form of Swahili used and taught at the UMCA mission in Zanzibar in the final decades of the nineteenth century. (Zanzibar Island is approximately 240 kms south of Mombasa by sea, off the Tanzania mainland.) The language engineering required to implement this decision was started in the mid-1920s and continued for the next 30 or 40 years. It is sometimes said that 'Standard Swahili' was based on the Zanzibar town dialect; this is not strictly true, but at the phonological level we are fairly safe in equating 'Standard Swahili' with the Swahili of Zanzibar Town (referred to in dialect descriptions as Kiunguja). However, because the vast majority of people who use Swahili in the countries of East Africa speak it as a second language, there is a great deal of variation at every linguistic level. This variation is, to some extent, controlled by the education system and by radio, and probably to some degree - at least in the major urban areas - by television. Although the medium of education in all urban and many rural primary schools in Kenya is English, Swahili is taught as a subject from the fourth year.

Returning to Mombasa itself, it is important to note that it has been an important sea-port for centuries; Vasco da Gama called there in 1498. It is likely that there has been a settlement on the site of the Old Town district at least since the 11th century, resulting from the southerly movement of the descendants of Perso-Arabian migrants who had arrived initially - and apparently spasmodically - at the northern end of the East African coast. A north-south movement down the Kenya coast is still discernible today - of young men, particularly from the Lamu area, seeking employment in Mombasa. Mombasans appear to have had for many years a strong sense of identity and a pride in their history - some of the events of which are enshrined in the considerable body of poetry which forms the literary heritage[3] of the northern half of the coastal strip. (The poetry was recorded in a modified form of the Arabic script, which is still in use today, although not taught in the municipal schools, who use the standard Roman orthography.) The poetic tradition appears to have begun in Lamu, in the north, and moved southwards, beginning to flourish in Mombasa in the early part of the 19th century. Topographical and economic factors tended to prevent extensive links between Mombasa and the other coastal settlements by land, but there were maritime trading links. This trade was dependent, until the advent of steam propulsion, on the NE and SW monsoon winds. It was these sailing winds which also formed the lands around the Indian Ocean into a trading unit, and was one reason for Mombasans' traditional focus of attention being seawards rather than inland. There were very few links with the interior (compared with those between Zanzibar and the interior) and even those trading links were organized through hinterland middlemen. Mombasa's present ethnic and linguistic heterogeneity dates largely from the early years of this century when the Mombasa-Lake Victoria railway line was completed and a new deep-water harbour was developed on the

west side of the island. The economic expansion brought about by both of these undertakings turned the population of Mombasa into a highly cosmopolitan one. The more recent tourist boom has also increased the number of migrant workers from other parts of Kenya, who obtain employment in service and other industries, as well as in government administration. This economic expansion has therefore reduced the proportion of indigenous Swahili-speakers in the population of Mombasa and - at a rough estimate[4] - they now form only approximately 10% of the total population of the municipality. Although members of this community are spread throughout most parts of the town the greatest concentration of them is in the Old Town district. Participant-observation in the Old Town community indicates that the incoming migrant workers (I shall refer to these as 'outsiders') are not generally regarded by the 'insiders' as prestigious and their Swahili is certainly not so regarded. 'Standard Swahili', unless spoken by inhabitants of the southern islands (Zanzibar and Pemba) or by Muslims from the Tanzanian mainland, tends to be associated, especially by older people, with inland non-literary culture and with lack of the sophistication of an Islamic way of life. In the last few years there has been an awareness among the small secondary school educated *élite* (mostly male teachers and university students) that the Mombasa 'dialect' is in danger of being eroded. One of the manifestations of this recognition is the publication of poems by the foremost local poet. The composition of poetry is still very much a flourishing art and cuts across differences of educational level and occupation. It should also be noted that 'outsiders' do not regard the 'insiders' as a prestige group, but see them as highly conservative and the Old Town as something of a backwater. As far as Swahili is concerned, there is no question in this situation of 'the acquisition of local rules by outsiders moving in' as in the Philadelphia situation referred to by Labov (1973b).

Mombasan Swahili ('Kimvita' in the dialect descriptions - see, for example, Stigand, 1915 and Whiteley, 1955) is characterized by a number of co-occurring phonological and morphological features (the small number of lexical differences between it and 'Standard Swahili'[5] need not concern us here). Some of these features are shared by the dialects north of Mombasa, and some by the southern group, and it is this which led Whiteley (1969) to refer to Kimvita as one of the 'bridge' dialects in and around Mombasa. Of these features two appear to have become stereotypes in the sense of Labov (1970): 'A small number of sociolinguistic markers rise to overt social consciousness and become stereotypes. There may or may not be a fixed relation between such stereotypes and actual usage.'

When a number of 'insiders' and 'outsiders' were asked to suggest how indigenous Mombasans might be recognized by their speech most of those questioned gave lexical examples to illustrate the difference between 'insider' and 'outsider' speech. The words given all contained the voiced and voiceless dental stop/postalveolar affricate distinction as in:

mʈele/mčele		'cleaned rice'	nɖia/nǰia		'road'
kiʈwa/kičwa		'head'	nɖoo/nǰoo		'come!'
-fiʈa/-fiča	(Vb)	'hide'	nɖaa/nǰaa		'hunger'
-ʈeka/-čeka	"	'laugh'	nɖe/nǰe		'outside'
-ʈukua/-čukua	"	'take'	mandano/manǰano		'yellow'
-ʈoma/-čoma	"	'burn'	-vunɖa/-vunǰa	(Vb)	'break'

The dental stop variants were associated by the respondents with 'insider' speech and the affricates with 'outsider' speech. (Note that the voiced variable is prenasalized.) These dental stops are also found in the Kiamu dialect, spoken in Lamu at the northern end of the Kenya coast.

We thus have the variables:

(ʈ/č) and (nɖ/nǰ)

The affricate variants are also the 'Standard Swahili' forms. Although 'outsider Swahili' in Mombasa - that is, Swahili used as a *lingua franca* - is highly heterogeneous, in this one area of the phonology, at least, we may treat 'outsider Swahili' and 'Standard Swahili' as synonymous terms. That is, Mombasan 'outsiders', whether speakers of other Bantu languages (eg Kikuyu, Kamba, Luhya) or non-Bantu languages (eg Luo), do not replace these affricates by stop consonants - nor by other obstruents, as far as observation indicates. However, in the speech of 'insiders' variation in respect of the stop/affricate alternation is observable, despite the popular image of the stereotypes, and it is this variation which provides the data for our discussion.

As well as expecting some variation in the speech of a community undergoing rapid sociocultural change we should also not be surprised to find that in the speech of a community with a history and geographical position like Mombasa's the identification of the loci of the variables in the lexicon is not a straightforward matter. For not all occurrences of /ʈ/ and /č/ provide a locus for the voiceless variable. Some items with /ʈ/ in Mombasan speech have alveolar /t/[6] in 'Standard Swahili', eg:

ʈaa (Momb)/taa (Std) 'lamp'
-ʈano (Momb)/-tano (Std) 'five'
-ʈaka (Vb)(Momb)/-taka (Vb)(Std) 'want'
ʈu (Adv and Adj)(Momb)/tu (Adv and Adj)(Std) 'simply, merely, just'

Foreign loans with /č/ do not provide a locus for the variable either, eg, čai 'tea', čerehani 'sewing machine', čaki 'chalk', -čapa (Vb) 'print'. Another set of items with invariable /č/ in both Mombasan and 'Standard' are not obvious loans and so may well be of Bantu[7] origin, eg:

-čafu	'dirty'	čumvi	'salt'
-čaŋga	'young, newly-born'	čuŋgwa	'orange'
-čemɲa (Vb)	'boil' (trans)	čenza	'mandarin orange'
-čelewa (Vb)	'be late'	kuča	'the whole (night)'

Finally, in connection with the (ʈ/č) variable, an environment is not provided by /č/ where it can be shown to be the surface realization of ki + /a/, /e/, /o/ or /u/, eg

ki + aŋgu > čaŋgu, ki + etu > čeʈu, ki + ombo > čombo, ki + uo > čuo.

This rule forces us to omit from consideration a large number of quite common words because *ki* is the nominal prefix and adjectival and verbal concord-marker of one class of nouns (class 7); these words have /č/ in both Mombasan and 'Standard'.

As far as the voiced variable is concerned only prenasalized forms provide a locus.[8] But there are two small sets of items with such forms which appear to be invariable, and must therefore be discounted as loci. Words in the first set have stop consonants which are dental in Mombasan and alveolar in 'Standard' Swahili, eg:

ŋḍiyo/ndiyo 'it is so', ŋḍipo/ndipo 'it is there/then'
and other inflected forms of ŋḍi-/ndi-
puŋḍe/punde (Adv) 'shortly, slightly, recently'
-peŋḍa/-penda (Vb) 'like, love'.

The second small set of 'exceptions' has an affricate in both Mombasan and 'Standard', eg:

nǰema 'very good, certainly'
nǰugu 'groundnuts'[9]

There is space only to mention the major and more obvious ramifications involved in the identification of the environments of the two variables. In attempting to establish these environments I worked from the tape-recorded data first and only used existing grammars, dialect descriptions and dictionaries as corroborative evidence where necessary.

As the dental stop variants seem to be popularly regarded as markers of 'insider' speech I am treating their occurrence in the data as evidence of 'insider' group identity. The data are recorded conversations from twenty-four adults encountered through my host family, the head of which was an influential resident or 'notable' (Gulliver, 1971). The speech of members of the network of a prestigious local family might reasonably be supposed to provide at least some information on current linguistic trends. Some of the subjects were encountered during one of their visits to the house. A great deal of neighbourly visiting goes on in this community and talking is a major leisure-time activity. These subjects were then asked to bring along a close friend of similar age on a subsequent occasion. A check was made that the members of each pair fulfilled the three criteria of Mombasan birthplace, Old Town residence and Swahili as mother-tongue; a concomitant attribute was, of course, Muslim religious affiliation.

The natural conversation of peers has been suggested by Labov (1973a) as a possible solution to the 'observer's paradox' and Gumperz and Blom (1971), who make the same point, also make an important statement concerning selection: 'Methodologically, self-recruitment of groups is important for two reasons. It insures that groups are defined by locally recognized relationships and enables the investigator to predict the norms relevant to their interaction.'

The adults fell into two age-groups: (i) early twenties to mid-thirties, (ii) over forty-five. (These groups will be referred to as B and C respectively; a group of young adolescents - Group A - will not concern us here.) There were six men and six women in each age-group, and it turned out, not surprisingly, that the members of each single-sex group of six knew each other; not all of them knew the other four as well as they knew their own partner, but they all had at least occasional contact with the others. In addition, the men and women in Group C knew each other, even if only slightly in a few cases. This was only partially true of Group B. In addition, most of the twenty-four knew one or more members of the host family.

The members of some pairs were linked by a number of role-relationships, eg two of the women in Group B were (i) close neighbours (ii) ex-class mates at school (iii) members of the same Muslim women's association. Many-stranded relationships of this kind usually imply frequency of interaction which, together with the fact that rumour and gossip are known to spread rapidly in this district, suggests a situation of the kind which tends to bring about homogeneity of norms and values (Boissevain, 1974). The whole group of twenty-four has a certain homogeneity anyway, since its members form part of one family's network, and live in the central and northern 'mitaa' (small neighbourhoods) of the Old Town district (as well as fulfilling all the criteria noted earlier).

Each person was involved in two kinds of dialogue - a conversation with his or her chosen peer and an interview with me; I spoke 'Standard' Swahili, conforming to the 'outsider' norm of non-accommodation to 'insider' speech. Note that this 'outsider' norm bears out Le Page's (1968) hypothesis in that 'outsiders' typically do not wish to identify with 'insiders'; in relation to the accommodation model (Giles and Powesland, 1975), which relies heavily on exchange theory (Homans, 1961), such non-accommodation could be explained in terms of costs to the speaker, ie identity-switch, which he is not prepared to pay. All the subjects, in common with most Old Town residents, have *some* contact with 'outsiders'; those who are in full-time employment have considerable contact, those who are not have much less. But anyone living in Mombasa - or any other urban area of Kenya - is exposed to non-coastal Swahili some of the time. Thus everyone has access to the major features characterizing the two Swahili codes, each of which is associated with particular cultural identities. The reason for calling the conversation with me an 'interview' (apart from ease of reference) is that the use of 'Standard'Swahili implies more formality than the use of Mombasan Swahili, whose use is associated with the home, neighbourhood and an Islamic way of life. Each dialogue consisted of 'informative and evaluative' discussion and personal narrative; all the material in each type of discourse was transcribed, up to a limit of 500 words per type of discourse, per person. (The interview also elicited fictional narrative but this is not relevant to the present discussion.) The topics were all concerned with local and personal matters. The conversation took place either in the house of one member of a pair or in the host family's house; I was not present during the peer-to-peer conversation.

As the loci for the voiced variable are far fewer than those for the voiceless one, it was decided to add the two sets of loci together to give larger totals. This seems justified on the grounds that the variables differ only in voicing and that for the respondents who had provided the original examples they form a 'sociolinguistic natural class'. The results are thus shown as percentages of /t̪/ + /nd̪/ occurring in the relevant loci. These group percentages are calculated from the individual raw scores.[10]

Putting the two age-groups together, to start with, and simply comparing the men's and the women's scores for /t̪/ + /nd̪/ we find, in Figure 1, that the popular image of Mombasan speech is most nearly realized in the speech of the women when talking to an 'insider' friend. A comparison of the two discussion scores in the women's speech indicates a difference of 35%, representing a considerable shift in the direction of 'Standard' Swahili when talking to an 'outsider'; the difference in the men's scores here is 23%. Whether the greater convergence of women towards the standard language in the interview is attributable to the interviewer's being a woman or whether this would have happened with a male 'outsider' it is not possible to say. Neither do we know whether a Kenyan 'outsider' would have triggered off a lower or higher percentage of Mombasan forms. The important point here is that the subjects were, in varying degrees, able to adjust to the interlocutor's form of speech *if they wanted to*.

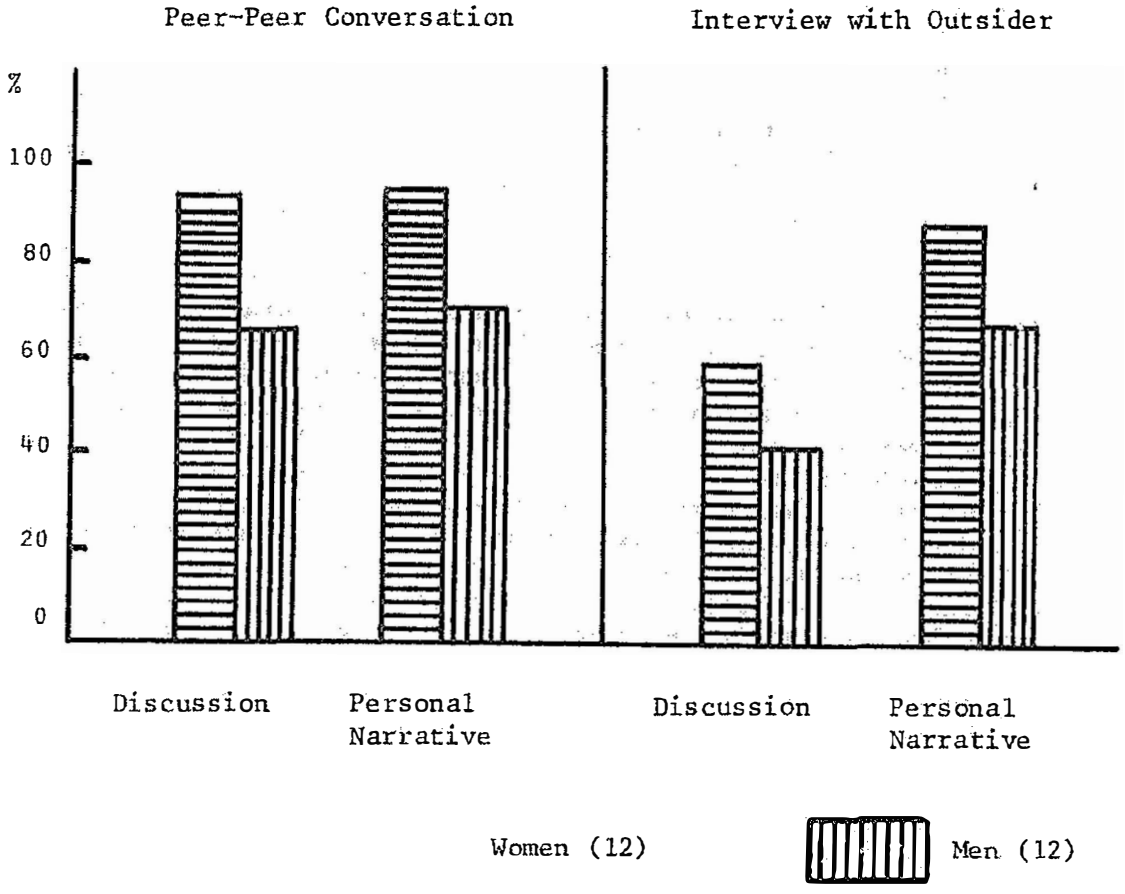

Figure 1: t̪ and nd̪ by sex

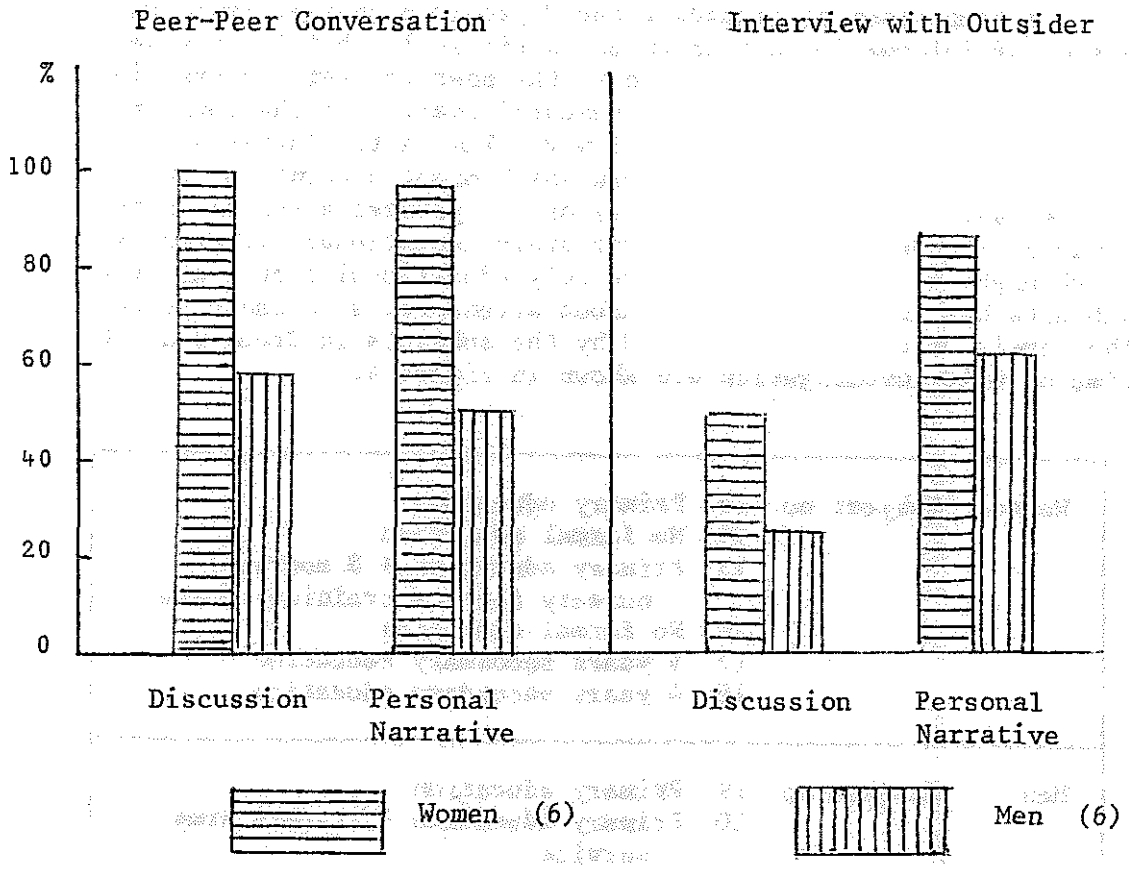

Figure 2: t_n and nd_{nn} by sex: Age-group B

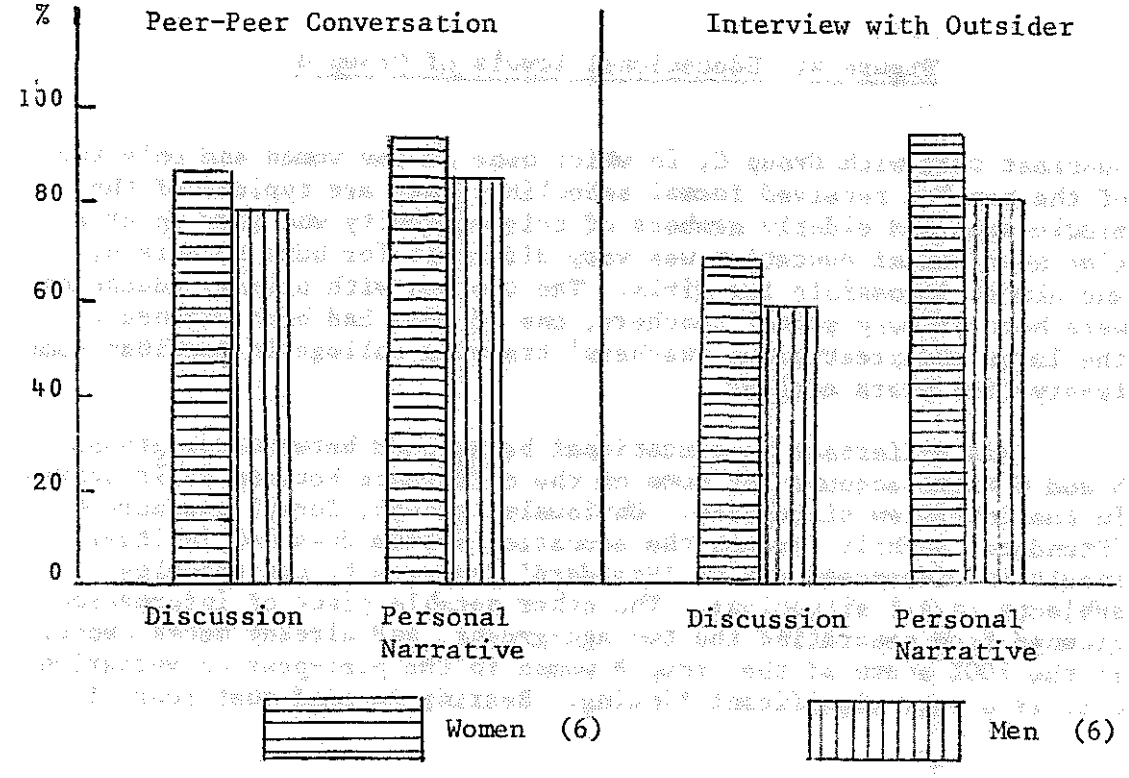

Figure 3: t_n and nd_{nn} by sex: Age -group C

A comparison of Figures 2 and 3 provides several important pieces of information not shown up in Figure 1, that is, the very high scores of the younger women in the peer-to-peer conversation and the greater shift towards 'Standard' Swahili of the younger group when talking to the interviewer. Taking the latter point first, we need to look for factors which might account for the greater ability (and willingness?) of the younger group to shift towards 'Standard'. There are certainly educational differences which might account for it. (The only educational factor all the subjects had in common was childhood attendance at Koranic school.) The levels of education achieved by the subjects in Group B at the time of this investigation are shown in Figure 4.

Women	Subject no	13	Primary education
		14	No formal education
		15	Primary education + 8 months nursery teacher training course
		16	No formal education
		17	4 years secondary education
		18	4 years secondary education
Men	Subject no	19	Primary education
		20	Primary education + 2 years army service
		21	4 years secondary education
		22	4 years secondary education
		23	4 years secondary education
		24	3 years secondary education

Figure 4: Educational levels of Group B

Contrast this with Group C, in which none of the women and only two of the men had received formal schooling; they are typical of the middle-aged and elderly members of this community who grew up at a time when formal education was very difficult for boys to obtain, and almost impossible for girls. The two men with primary education were both primary school teachers, one of whom had been trained at the large and prestigious teachers' training college in Zanzibar some twenty-five years earlier.

This difference in educational background between age-groups B and C might account for *some* of the difference between their scores in the interview discussion. Obviously, though, formal exposure to 'Standard' Swahili through the education system does not in itself result in widespread use of 'Standard' features by the schooled subjects in all situations. The other notable piece of information gleaned from separating the two age-groups, and already noted above, is the 100% score of the Group B women in the peer-peer conversation; this is a very significant finding. Bearing in mind that four of

these women had had formal schooling, two of them having successfully completed a four-year secondary-school course, one would not have been surprised by a much lower score than this. The scores of these women in both types of speech situation are - apart from the discussion in the formal interview - much closer to those of Group C women than one would have expected, given the difference of educational background. This can only support the contention that these women have a much greater sense of 'insider' group identity than the male subjects in the same age-group, and it perhaps also indicates more self-confidence. The fact that the Group C women's scores, although high, nowhere reach 100% underlines the fact that an acquaintance with the education system is not necessary in order to acquire *some* knowledge of 'Standard' Swahili features; simply living in Mombasa - together with access to radio and television and comparatively easy access to Nairobi and other inland towns - is sufficient. One item of interest to emerge from the graphs in Figures 1, 2 and 3 is that in the interview the nature of the discourse in the 'personal narrative' section took precedence over the 'interviewer-interviewee' (or 'outsider-insider') relationship; these narratives were uninterrupted accounts of events in which the subjects had been personally involved.

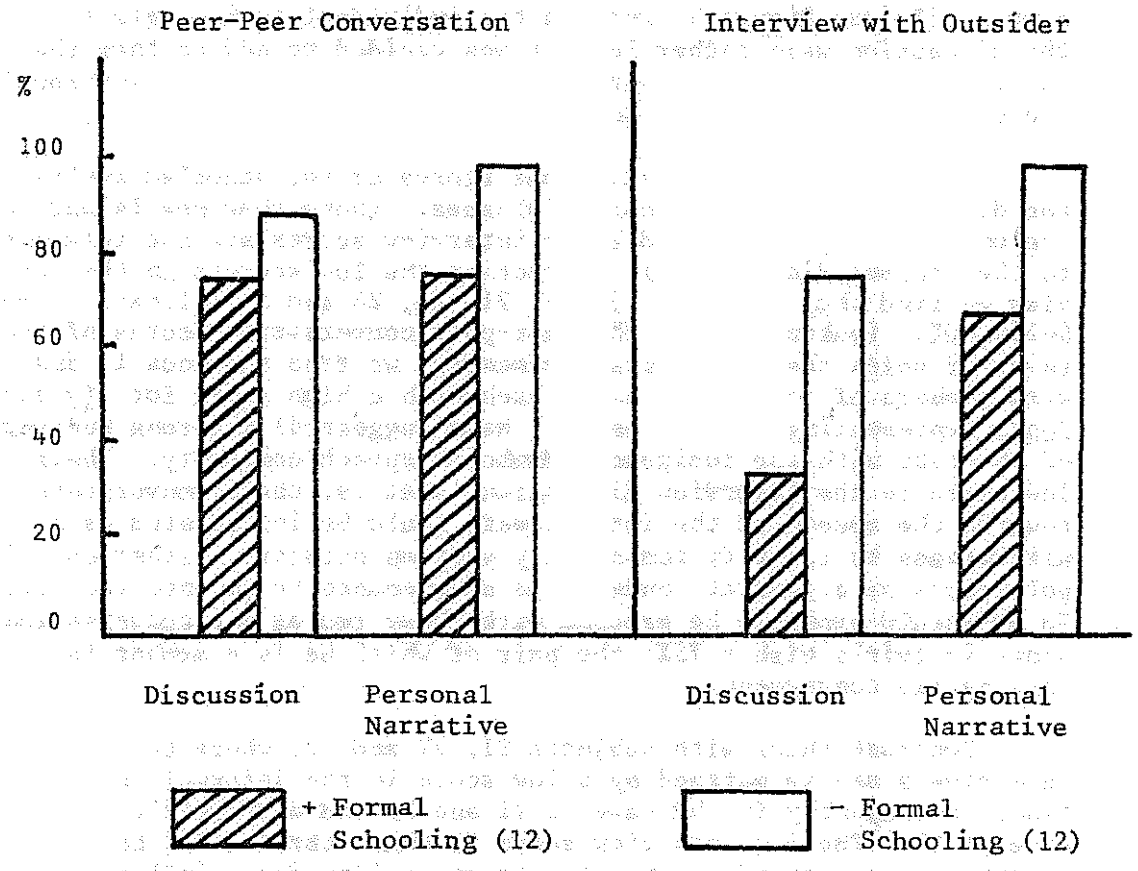

Figure 5: $\bar{x}$ and $\bar{x}_d$ by education

Taking all the adults together, and making a comparison between schooled and unschooled subjects, we find in Figure 5 that the link suggested above - between the marked shift towards 'Standard' forms in the interview discussion and the acquisition of formal schooling - is now clear. If we are to explore a little further the nature of the difference between the discussion scores in the two dialogues we had better leave out of account the unschooled subjects since their *ability* to shift towards 'Standard' Swahili may be less than that of the schooled group (see Le Page's third and fourth 'riders' above).

So, dealing only with the schooled subjects (eight men and four women) we may ask whether, in the interview, a low score represents convergence to the outside interviewer's speech or a weak sense of 'insider' group identity. Obviously, there is no way of knowing this from the group interview scores alone; we need individual scores, and they will have to be compared with the individual scores achieved in the informal peer-peer discussion. The method employed for the collection of the data was geared towards making eventual statements about group behaviour, and the figures involved, ie the *group* scores, are adequate for this. However, the *individual* loci-totals are rather small in some cases, and so any conclusions drawn from the comparison of individual scores must be tentative; I am simply using them to illustrate an approach which seeks an explanation of variation and potential linguistic change at its locus, ie the dyadic encounter. The group scores for the discussion and narrative in the peer-peer dialogue turned out to be exactly the same - 73% (see Figure 5) and, as the individual loci-totals for the discussion were rather low, it was decided to add to them the individual totals for the narrative when calculating the individual scores; this gave slightly more respectable totals.[11]

Figure 6 shows the individual scores of the schooled adults in the discussion section of both dialogues. (Note that nos 14 and 16 are unschooled partners and their interview scores are not relevant to the present discussion.) Extracting the low scorers in the interview we find that subjects 13, 17, 21, 22, 24 and 32 all have scores below 40%. Looking now at the peer-peer conversation scores of the pairs of which these subjects are members we find that nos 13 and 17 were members of convergent pairs each with a high score for /t̪/ and /n̪d̪/, representing (in the terms I have suggested) a strong feeling of identity with the indigenous Mombasan speech community. Their low score in the interview discussion, that is, their convergence towards the speech of the interviewer, could be interpreted as a willingness to identify temporarily with an outsider, either out of politeness or a general tendency to accommodate to an interlocutor. No 24 should probably be grouped with these two as his conversation score is fairly high - 75%; the pair of which he is a member is only partly convergent.

Contrast these with subjects 21, 22 and 32, where the low interview score is matched by a low score in the informal conversation, convergently in the case of 21 and 22 and divergently in the case of 32. The low interview score in these three cases then, appears to be part of a general pattern of 'Standard'-influenced speech and weak group-loyalty; it is *not* evidence of accommodation to the interviewer's speech.

Subject no	Conversation with peer: discussion	Interview with 'outsider': discussion
	%	%
f 13	100	10
f 14 (unsch)	100	
f 15	100	61.5
f 16 (unsch)	100	
f 17	100	37
f 18	87	64
m 19	100	75
m 20	67	100
m 21	0	7
m 22	0	0
m 23	42	54
m 24	75	30
m 31	100	100
m 32	25	13

Figure 6: Comparison of schooled adults' individual /t̪/ and /nd̪/ scores in informal conversation and formal interview discussions

The high scorers in the interview are subjects 15, 18, 19, 20 and 31. Of these 15, 18, 19 and 20 are members of convergent pairs, all with high or fairly high scores, although the convergence of 19 and 20 is not very marked. (Convergence is, of course, what we would expect in this kind of conversation between friends, where no transactional element is involved.) No 31 is a member of a highly divergent pair.[12] He and his partner, both aged about fifty, are the oldest of the schooled adults and it may be that the amount of divergence evident in their scores is the result of the kind of confidence that comes with maturity; each is very much his 'own man'. The five high-scorers look as though they identify strongly with their community, and this sense of identity carries over into dialogue with an outsider. No 23 has not been mentioned; it does not seem justifiable to group him with the high scorers and we had better leave him where he has placed himself – sitting on the sociolinguistic fence.

From the point of view of the linguistic structure of the 'ways of speaking' along the East African coast which have come to be called 'Swahili' we may, on the basis of the foregoing exploration of variation involving a minute part of that structure, wish to agree with Bickerton (1975): 'The amount of variation in any given language may vary from epoch to epoch, but is always present in

some degree, and continually shifts its locus; if the feature that is variable today was often invariant yesterday, it is equally true that today's invariant was yesterday's variable.' We do not know the extent of 'yesterday's' variation in respect of particular phonological or other features of Swahili but there are certainly hints of considerable variation by earlier investigators, eg Krapf (1844), who learned Swahili in Mombasa and wrote its first grammar: '... the Sooahelee is so vague and differently spoken by different persons that I frequently was inclined to despair ...' and Stigand (1915): 'The great bulk of the inhabitants of Mombasa at the present day, talk a language which is neither pure Mombasa nor pure Zanzibar.' However, if we want to discover why what might have been yesterday's invariant has become today's variable we need to observe people talking to a variety of acquaintances, at different points in their personal networks - from their cluster of close friends (Boissevain's 1974 '1st order zone') as well as from their casually-encountered and perhaps temporary acquaintances (in the Mombasa context these are likely to be 'outsiders'). The point is that the links in networks are potential communication channels, and an examination (on a larger scale than the one described) of the linguistic behaviour of people linked in this way can help to plot the mechanism of code-switching (used here in the broadest sense of the term) and thus uncover the sources of potential linguistic change in a community.

Subjects 21, 22 and 32, who had low scores in *both* dialogues, appear to have a general pattern of 'Standard'-influenced speech - a likely indication of a willingness to accept the norms associated with the use of 'Standard Swahili' (which is increasingly identified with government and a non-coastal urban way of life). It is these people and others like them whose *total* linguistic range is changing, who are the instigators and carriers of potential linguistic change. The linguistic change is the merger of /ʈ/ with /č/ in Mombasa Swahili, thus bringing about congruity with the set of voiceless stop and affricate phonemes of 'Standard' Swahili (see footnote 6) and the merger of [ɖ] with the [ǰ] allophone of 'Standard' /ɟ/ (see footnote 8). Whether this potential change becomes actual is dependent on a number of social and economic factors and cannot therefore be predicted. It is people like these - men with secondary-school education or post-primary training and a positive attitude to society outside their own speech community - who are the most likely to move away from Mombasa if there are insufficient opportunities for suitable employment there. However, they will only move away if there is congenial employment elsewhere. If this happens and if the southward drift of less-educated and less-skilled men from the Lamu area continues, thus reinforcing the northern features which Mombasan Swahili shares with Lamu speech, then perhaps the existing situation will continue for some time. As far as the present is concerned, if the women in this study are at all representative it certainly looks as though it is women, rather than men, who are preserving the more obvious markers of this speech community.

FOOTNOTES

1. The data referred to in this paper were collected during April-September 1973. I am grateful to the University of York, which granted me study leave, to the Social Science Research Council for financial assistance and to the Kenya Government for granting me permission to carry out linguistic research in Mombasa. This is an expanded version of a paper presented to the Research Seminar on Sociolinguistic Variation at West Midlands College in 1976 and circulated in mimeo, under the present title, in 1977. I am grateful to Professor R B Le Page for commenting on the earlier version.

2. The area of the Old Town is approximately 1 sq km; the population at the time of the 1969 census was 19,959. The choice of this district was influenced by Whiteley's (1955) observations: 'This Afro-Arab group constitutes the core of the Kimvita-speaking community, and is located mainly in the old part of the town overlooking the dhow harbour on the northern shores of the island.'

3. The introduction by Shihabuddin Chiraghdin, to Ahmed Nassir's 1971 collection of poems (see bibliography) sums up very well the sentiments of the Swahili *élite* towards the poetic tradition and the Mombasa dialect itself.

4. This estimate is based on the relevant figures for Mombasa Municipality in the 1969 Kenya Population Census. The totals for those groups which are *likely* to be native-speakers of Swahili are combined, thus:

Swahili/Shirazi	3,920
Bajun	5,328
Arab (Kenyan)	13,784
	23,032

The total population of Mombasa Municipality is given in the same census as 247,073.

5. Any discussion of lexical differences between Mombasan and 'Standard' Swahili would, in any case, be hampered by the eclectic nature of the choice of entries for the Standard Swahili Dictionary (1939).

6. Mombasan speech also has alveolar /t/. This and /t̪/ can both be assigned phonemic status on the basis of a small number of minimal pairs. The voiceless stop and affricate phonemes of the two dialects are thus:

Kimvita	p	t̪	t	č	k
Kiunguja (Standard)	p		t	č	k

7. Three of them almost certainly are, ie kuča, the infinitive form of -ča (to dawn); -čelewa, a derivative of the same verb-root; and -čače, (see Guthrie, 1970: Vol III, Comparative Series 1034 and 1047). The provenance of the others is uncertain.

8. This post-nasal voiced segment is in allophonic variation with the implosive palatal stop [ɟ] which occurs in all other environments, ie word-initally and intervocalically, in both Kimvita and Kiunguja. The voiced variable, unlike the voiceless one, is thus at the sub-phonemic level. /nd̠/ and /nǰ/ will be shown between slashed lines, however, for ease of reference and to indicate *sociolinguistic* parity with /t̠/ and /č/.

9. Whiteley (1955) gives also kanǰa 'plaited coconut palm fronds', nǰana 'a kind of fish' and nǰuga 'small bell' as being invariable, but these did not occur in my data.

10. The mean number of loci per single-sex age-group of six is as follows:

	Conversation	Interview
Discussion	18	59
Narrative	35	23

11. The individual totals for both types of discourse in each dialogue range from 23 to 1. The single occurrence of 1 is the 'total' of subject no 31 (one of the Group C men) for the discussion in the peer-peer conversation. He scored 100% in *all* types of discourse (ie including the fictional narrative not discussed here); in other - unrecorded - conversations he was never heard to use 'Standard' forms in these environments. In any case, he, as a high scorer in both dialogues, is not relevant to my final remarks on the carriers of potential linguistic change.

12. Nos 31 and 32 are the two Group C primary-school teachers mentioned earlier. No 32 is the man who was trained in Zanzibar; he teaches in a school on the mainland (whereas no 31 teaches in the boys' school nearest to the Old Town district) and has had at least one book of children's stories published - in 'Standard' Swahili. These facts indicate a positive attitude to the 'outside world' and to the standard language, and this is reflected in his speech.

REFERENCES

Bickerton, D
1975 Dynamics of a Creole System, Cambridge: U.P.

Boissevain, J
1974 Friends of Friends: Networks, Manipulators and Coalitions, Oxford: Blackwell.

Boissevain, J and J C Mitchell (ed.)
1973 Network Analysis: Studies in Human Interaction, The Hague: Mouton.

Giles, H and P F Powesland
1975 Speech Style and Social Evaluation, New York: Academic Press.

Gulliver, P H
1971 Neighbours and Networks, Berkeley: California U.P.

Gumperz, J
1976a 'Social network and language shift', Working Paper 46, Language Behavior Research Lab. Berkeley: University of California.

1976b 'The sociolinguistic significance of conversational code-switching'. Unpublished MS.

Gumperz, J and J-P Blom
1971 'Social meaning in linguistic structures: Code-switching in Norway', in AS Dil (ed.) Language in Social Groups: Essays by John J Gumperz, Stanford: U.P., 274-310.

Guthrie, M
1970 Comparative Bantu Vol III, Farnborough: Gregg Press.

Homans, G C
1961 Social Behavior, New York: Harcourt, Brace and World.

Kapferer, B
1973 'Social network and conjugal role in urban Zambia: Towards a reformulation of the Bott hypothesis', in Boissevain and Mitchell (ed.): 83-110.

Kenya Population Census 1969 Vol I
1970 Kenya Ministry of Finance and Economic Planning (Statistics Division).

Krapf, L
1844 Copy of letter to R P Waters, U S Consul in Zanzibar. Mombasa, 2 September 1844, Folio 2 of File CA5/016 (Krapf's correspondence with CMS headquarters). London: Church Missionary Society Archives.

Labov, W
1970 'The study of language in its social context', Studium Generale 23: 66-84.

1973a 'The linguistic consequences of being a lame', Language in Society 2.1: 81-115.

1973b 'Where do grammars stop?', 23rd Annual Round Table on Linguistics and Language Studies, Georgetown: U.P., 43-88.

Le Page, R B
1968 'Problems of description in multilingual communities', Transactions of the Philological Society: 189-212.

Milroy, L and S Margrain
1978 'Vernacular language loyalty and social network', Belfast Working Papers in Language and Linguistics 3.

Mitchell, J C
1973 'Networks norms and institutions', in Boissevain and Mitchell (ed.): 15-35.

Nassir, A (ed. S Chiraghdin)
1971 Malenga wa Mvita, Nairobi: Oxford U.P.

Nicholls, C S
1971 The Swahili Coast: Politics, Diplomacy and Trade on the East African Littoral 1798-1856, London: George Allen and Unwin.

Polomé, E C
1967 Swahili Language Handbook, Washington DC: Center for Applied Linguistics.

Slavikova, M and M A Bryan
1973 'The case of two Swahili dialects', African Language Studies: 53-81.

Standard Swahili-English Dictionary
1939 Oxford: U.P.

Stigand, C H
1915 A Grammar of Dialectic Changes in the Kiswahili Language, Cambridge: U.P.

Thoden van Velzen, H U E
1973 'Coalitions and network analysis', in Boissevain and Mitchell (ed.): 219-50.

Whiteley, W H
1955 'Kimvita: an enquiry into dialectal status and characteristics', Journal of the East African Swahili Committee 25: 10-39.

1969 Swahili: The Rise of a National Language, London: Methuen.

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CODE-SWITCHING IN LADY CHATTERLEY'S LOVER

In multilingual societies it has been suggested that the use of all the available linguistic forms is a prerequisite for full participation in the community (Gumperz, 1964: 206-07). That is, in a French/English bilingual community, it is necessary for members of the community to know and speak both languages if they are to obtain the maximum status and benefits possible in that community. Recent research in bilingualism, in fact, has focused on exactly *how* this code-switching actually takes place. Innumerable interesting questions present themselves to linguists in this regard, usually involving the when's, the how's, the where's, the who's, the how much's of such switching. The assumption, of course, is that code-switching is not random and that certain social conditions create the need and dictate the procedures for engaging in it. Gumperz goes so far as to state that '... alternation does carry meaning' (1971: 316) and that the very act of switching serves social and semantic functions.

The concept of 'communicative competence' (Hymes, 1967) involves the recognition that speakers have the ability to use their speech varieties for specific functions, social or linguistic. The actual investigation of communicative competence in various language contact situations has been relatively recent and not carried very deeply in many language learning situations. For example, the effective learner of a second language may well acquire effective or native-like phonology or grammar in a second language but never acquire a working knowledge of the crucial language functions that will enable him to open and close conversations, infer meaning from contexts in which that meaning is not explicitly carried by the lexicon, interrupt, cajole and many other language functions which have been shown to be critical measurement points of communicative adequacy. Likewise, little or no attention has been given to the question of the social benefits which can accrue to the foreigner who preserves certain phonological or grammatical flavourings from his native language as he acquires a second tongue. It appears even heretical to suggest that a second-language learner may be tolerated in a different way or even tolerated preferentially if the native speakers are given adequate signals of his foreignness.

Research on alternation between codes has indicated rather clearly that such behaviour is rule-governed (Ervin-Tripp, 1972). The regularities which are shown to result from such alternation relate to factors which make up the individual speech events. Such factors include topic, code, situation and participants (Hymes, 1972: 58-65).

Topic (what is being talked about) has been discussed in the context of code-switching on several occasions (Ervin-Tripp, 1972; Fishman, 1972; Gumperz, 1964; Hymes, 1972) and empirically analysed

by Ervin-Tripp (1964), by Gumperz and Blom (1971) and Strick (1973). In each case the research related code-switching to dominant and second languages as a function of topic.

Ervin-Tripp (1964) investigated the effect of variation of topic and receiver on switching between Japanese and English. Interviews with Japanese immigrants were conducted by Japanese and Caucasian Americans. If the listener was Japanese and the topic related to American culture, the speaker seemed to have no difficulty if speaking English. On the other hand, if the interviewer was Japanese and the topic related to Japanese culture, the conversation in English was less perfect syntactically, less fluent and was punctuated by Japanese borrowings. Ervin-Tripp concluded that changes in topic and listener had marked effect on the formal features of speech (1964: 97).

Similarly in their research on code-switching in the speech of California Chicanos, Gumperz and Hernandez (1971) found that a socially identifiable topic often determined which code the bilingual would select. For example, whenever Chicano identity was an underlying theme, Spanish was used. In an earlier study of the effect of topical variation on code-switching in a small Norwegian village (1964 and 1971) Gumperz concluded that of the two dialects spoken in that village, the local one was preferred in issues related to community identification while standard Norwegian was used in topics which were more national in scope. The effect of topic was carried even further when Gumperz and Blom indicated that code-switching would not occur in friendly gatherings of people who composed a network of local relationships, even if topic is varied. On the other hand, if both local and non-local relationships obtain, code-switching could occur based on topical variation. Thus the speakers' verbal strategy of conveying social information is revealed by switching from the local dialect to the standard.

The systematic study of code-switching by linguists usually involves the following techniques: anonymous observations (Gumperz, 1964: 171-72; Gumperz and Blom, 1971), individual informant interviews (Labov, 1966; Shuy, Wolfram and Riley, 1968) and small-group elicitations or discussion sessions (Gumperz, 1971; Labov, 1970: 46-49). As far as can be determined, little or no work has been done by linguists in studying the already written observation of code-switching by sensitive writers. The hypothesis of the present study is that a sociolinguistic theory of code-swithcing can be applied not only to the examination of real conversational data, whether elicited surreptitiously or in interview contexts, but also to the written representation of such real conversational data by competent authors. By applying what is known about code-switching as rule-governed behaviour we should be able to determine the degree to which a given author consistently represents this rule-governed behaviour and, to the extent which he is consistent or inconsistent, evaluate that author's innate sociolinguistic effectiveness. For the sociolinguist, the information being examined (a novel, a short story or a poem) may be little more than a new batch of data through which he can crank and test his sociolinguistic theory and machinery. To the literary critic, however, the results of recent research in communicative competence in general, and in this case, code-switching in particular

offer a new and objective instrument for analyzing an author's consistency in representing reality, for examining a writer's subtle shifts of intention or indications of characterization. It has been observed that great authors write better than they know. One would assume from such a statement that writers not only internalize sociolinguistic rules, language functions and formal language knowledge, but that they also make use of such features without really being able to say what it is they are doing as they do it. This is not surprising to linguists who have long marvelled at how well children acquire their native language without having the foggiest notion of how to describe this knowledge to others. It might be argued, of course, that such behaviour is not really writing better than an author knows (depending on how *knows* is defined); it is, rather, writing better than his ability to describe his ability to write. A sociolinguistic analysis of literature promises nothing to the writer in this case, but it does offer a theory and methodology for discussing what it is that a writer does and how well he does it.

In order to illustrate the usefulness of the knowledge of code-switching in a literary context, we have selected D H Lawrence's novel Lady Chatterley's Lover, which contains one of the classic literary instances of dialect-shifting. Mellors, the gamekeeper in the household of Lord and Lady Chatterley, speaks what Lawrence describes as broad Derbyshire dialect on many occasions. On the other hand, he is also known to speak a rather standard version of English, perhaps as a result of his being '... attached to some Indian colonel who took a liking to him' when he served as a lieutenant in the army. We get a glimpse of this linguistic situation when Lady Chatterley asks her husband: "How could they make him an officer when he speaks broad Derbyshire?". To this Sir Clifford replies: "He doesn't ... except by fits and starts. He can speak perfectly well, for him. I suppose he has the idea if he's come down to the ranks again, he'd better speak as the ranks speak." This speech certainly reflects the observations of sociolinguistic researchers on how social information is revealed by language switching. It is also a strong indication that Lawrence was probably consciously aware of this sociolinguistic principle. What remains is to observe how well he carried it out in his representations of the speech of his characters.

The codes involved in Lady Chatterley's Lover are assumed to be two homogeneous and clear-cut dialects of English. This raises one theoretical and terminological problem for linguists, whose definition of code historically has usually meant two separate non-mutually intelligible language systems. It is not at all clear that Gumperz uses the term this way, however, and a good case can be made for lumping code-switching with the dialect-switching (sometimes called style-shifting) in terms of their roles and effects. Regardless of exact terminological precision, it appears that the two language systems in contrast are a kind of standard English versus a broad Derbyshire dialect. Lawrence is relatively clear in the instances in which he wishes his reader to perceive the dialogue as being in the latter dialect. The accuracy of his representation will not be at issue here but suffice it to say that phonology of this dialect is represented in spellings (*yer* for

you, *waitin'* for *waiting*, *'adn't* for *hadn't*, *a'* for *all*, *Ah* for *I*, *pleece* for *place*, *ax* for *ask*, *abaht* for *about*, *ma'es* for *makes* etc). Occasionally, the orthographic representation appears to be mere eye-dialect, as in *th'* for *the*, *du* for *do* and *anuther* for *another*, but generally speaking, Lawrence represents broad Derbyshire pronunciation rather consistently. The grammar of the dialect is predictably non-standard, with double negatives (*Sir Clifford 'adn't got no other key then?*), non-standard verb usages (*Ah thowt it wor ordinary*) and local syntactic forms (*'Appen Sir Clifford 'ud know*). The standard dialect is characterized primarily by a regularity of orthography and grammar, but additionally by the absence of the marked forms of broad Derbyshire such as those noted above. In the minds of writers, as apparently in the minds of most speakers, standard is primarily the absence of stigmatized forms (Shuy, 1969).

In terms of the *settings* and *participants* involved in the switching, Mellors speaks only standard English to Sir Clifford, Sir Malcolm and Mrs Bolton. He speaks standard English to Hilda until she insults him, at which point he answers her in dialect. He also speaks dialect to Hilda, primarily when she is a visitor in his house. He speaks local dialect consistently to his dog, and to his penis on the occasions which he addresses it as a person. It is only when Mellors switches in speaking to Lady Chatterley that clarification is required.

As noted above, *topic* also is an essential factor in code-switching. Consistently throughout the novel certain topics are discussed by Mellors in standard English only (three separate occasions), Mellors discusses it only in standard. The same can be said for the topics of sex (mutual orgasm, his libido, and any memories of their past sexual experiences), three discussions about their philosophies of life, Mellors' personal background, the topic of divorce (four times) and discussions about what constitutes good English. Three times the topic of Mellors' first wife is discussed in standard English and once in dialect. Likewise, Mellors discusses his work as gamekeeper once in standard and once in dialect. More personal topics, however, are generally discussed in dialect, including his relationship with his daughter, the general topic of children and any philosophical discussions relating to the hardship of the life of a peasant.

In terms of *language situations* or functions, the consistent contrast between standard English and dialect in Mellors' speech is maintained. Introductions, conversational openings, conversational closings, insults, invitations to sex and rejection of sexual overtures are in standard English, while all representations of meal-time conversation, talk during love-making and post-intercourse afterglow conversations were in dialect.

The novel contains twelve major speech events, separated by descriptive phrases and authorial narrative. The term *speech event*, as used here, will refer to extended conversations in which switching is potential or actually takes place. The four major ingredients for code-switching will be noted for each speech event.

Speech Event 1. (in Chapter V)

Topic: Sir Clifford introduces Lady Chatterley to Mellors.
Participants: Sir Clifford, Lady Chatterley, Mellors.
Setting: Sir Clifford's home (Wragby).
Code: Standard English (SE).

One hint of switching or potential switching comes from Lawrence, who notes that Mellors corrected himself on one occasion:

"But you've been here some time, haven't you?"
"Eight months, Madam - your Ladyship!" he corrected himself calmly.

In addition, Lawrence comments on the nature of Mellors' speech:

"And do you like it?"
"Why yes, thank you, your Ladyship. I was reared here ..."
His voice on the last words had fallen into the heavy broad drag of the dialect ... perhaps also in mockery because there had been no trace of dialect before.

Speech Event 2. (in Chapter VIII)

Topic: Discussion about the hut in the woods. It was used by Mellors but Lady Chatterley found it a nice place to come and sit when taking a walk. They discuss whether or not she can/should use it and whether or not he should stop using it as a work centre.
Participants: Lady Chatterley and Mellors.
Setting: At the hut in the woods.
Code: Primarily vernacular. Mellors uses SE only in response to Lady Chatterley's question about why he should worry whether or not she needs the hut to herself:

"Why should I take any notice of you and your being here or not? Why is it important?" ...
"It's not, your Ladyship. Not in the very least."

Lady Chatterley is not satisfied with this answer and pursues the point again:

"Well why then?"

to which Mellors changes the subject:

"Shall I get your Ladyship another key then?"

She responds with an emphatic *no*, indicating clearly that it is impudent of Mellors to think that she would even want to come there. At this point, Mellors returns to the vernacular:

"Ah'll get it anyhow. We'd best 'ave two keys ter th' place."

Lady Chatterley is furious. She calls him insolent, and Mellors, still in the vernacular, denies that he ever had any untoward intentions and plays the role of the ignorant and humble worker:

> "I niver meant nuthink. Ah on'y thought as if yo' come 'ere, Ah s'd 'ave ter clear out, an' it 'd mean a lot of work, settin' up somewheres else ..."

Lawrence comments, as this speech event ends, that Lady Chatterley is in total bewilderment:

> She was not sure whether she had been insulted and mortally offended, or not. Perhaps the man really only meant what he said; that he thought she would expect him to keep away. As if she would dream of it! And as if he could possibly be as important, he and his stupid presence.

The effect of Mellors' switching is clear. He uses SE in the role of chastized servant, similar to a butler sterectype, offering SE noises with little more than functional rather than semantic intent (a sort of "As you wish, Madam - Whatever Madam desires"). In such a role, he can perfectly legitimately assume the accompanying role of professional ignorance. Just as it was within Mellors' character of ignorant, humble servant to ask if she wanted a key, it was also within his province as ignorant, humble gamekeeper to reject her denial and say that he'd have one made anyway. The passive-butler, SE-speaking role would not permit this sort of ignorance. It would have required a passive acceptance with an "As you wish, your Ladyship". To force the key upon Lady Chatterley he had to switch servant rolls to that of the even more ignorant and humble outdoorsman, who would not have to accept passively. The gamekeeper could legitimately misunderstand, even to the extent of countermanding her wishes.

Small wonder that Lady Chatterley left the setting in confusion. She had been dealing with at least two surface-role representations (formal and informal servants) as well as the real Mellors, whoever he might turn out to be. The role-shifting has served Mellors well. At the onset of this conversation he had dazzled her with what Lawrence called 'the fog of the dialect'. Her reaction was to question:

> "Why don't you speak in ordinary English?"

to this Mellors responds:

> "Me! Ah thowt it *wor*' ordinary."

In contrast to speech event number 1, the second speech event is vernacular-dominated. The topic shifts from stylized speech functions of event number 1 to the local topic involving the nature and responsibilities of the gamekeeper's work. The setting shifts from Sir Clifford's house in event number 1 to the gamekeeper's work-hut in number 2, a more amenable place to the use of vernacular.

The participants also vary, and Mellors' pattern of never switching in the presence of Sir Clifford is firmly established. From the perspective of the sociolinguist, these events show realistic predictability. The scenes described by the novelist might well have been tape recordings of real-life conversations. From the perspective of the literary critic, this exercise offers a useful evaluation instrument for the effectiveness of a writer in reflecting the reality he attempts to depict. The performance of any creator of literature is a struggle with form. Writing demands conformity and restricts freedom in the interests of the formal mechanism. The illusion of reality in conversation is one of the measures of authorial validity. Lawrence might have narrated these scenes without conversation, using the vision of the outsider. Instead, he chose to use the vision of the insider, and, in doing so, makes himself vulnerable to the charge of artificiality or inconsistency.

Speech Event 3. (in Chapter X)

Topic: Mellors' obtaining a key to the hut for Lady Chatterley and a brief discussion of the hut's function (involving the hens).
Participants: Mellors and Lady Chatterley.
Setting: At the hut in the woods.
Code: SE, switching to vernacular.

Again, the hens, especially their symbolization of warmth, life and feminine tenderness, affect Lady Chatterely greatly. She cries. Mellors is moved, touches her shoulder and moves his hand down her back, then invites her into the hut. Inside, he instructs her to lie down and then he lies down beside her. Their first intercourse follows, with all conversation in standard English. It is not until they are past that Mellors begins to use vernacular. They discuss the complications of their behaviour and the concept of love. Then Lady Chatterley asks:

"But you don't hate me, do you?"

Mellors responds:

"Nay, nay," in Derbyshire dialect.

Speech Event 5. (in Chapter X)

Topic: Sex, being found out, their relationship.
Participants: Lady Chatterley and Mellors.
Setting: At the hut.
Code: Vernacular, switching to SE, back to vernacular, back to SE, back to vernacular and back to SE.

The scene opens with Mellors coming upon Lady Chatterley who was sitting in the hut:

"You come then," he said, using the intonation of the dialect.

Mellors continues to use the dialect throughout their discussion of whether or not people will discover them. He introduces sex in standard but returns to dialect during and after their intercourse. Lawrence exerts the authorial perspective only once, when he notes that Lady Chatterley resented the dialect: 'His "Tha mun come" seemed not addressed to her, but some common woman.' As they discuss her leaving, however, Mellors returns to standard: '"It's quarter past seven," he said, "you'll do it." He had changed his voice', Lawrence reports. But as they are parting, he lusts for her touch again, and says so in dialect. They embrace, then separate, and his leave-taking returns to standard:

"Good-night, your Ladyship."

Speech Event 6. (in Chapter X)

Topic: Mellors' libido and their own sex act.
Participants: Lady Chatterley and Mellors.
Setting: On the road.
Code: Entirely in SE.

Mellors intercepts Lady Chatterley and almost forcibly takes her into the woods to a clearing suitable for sex. She receives him passively, but, uncontrollably, she climaxes almost in spite of herself. They discuss this phenomenon entirely in SE.

Speech Event 7a. (in Chapter XII)

Topic: His work, her proposed trip to Venice, their sex experience, their relationship.
Participants: Lady Chatterley and Mellors.
Setting: Mellors' cottage.
Code: SE opening comments, falling into dialect at the meal ("Shall Y'ave something?"), returning to standard when Mellors discusses his work (Lawrence notes: 'He spoke cold, good English') and when they talk about her proposed trip. Mellors remains in standard as they discuss their past sex experiences and the possibility of having used each other. The sex invitation, as usual, is in standard, but they decide to go to the hut rather than to have intercourse at his cottage after dinner. Lady Chatterley then left and went home.

Speech Event 7b. (in Chapter XII, immediately following Speech Event 7a)

Topic: The hens, sex introduction, afterglow small talk.
Participants: Lady Chatterley and Mellors.
Setting: At the hut.
Code: After greeting in standard, Mellors invites her into the hut in dialect ("Shall us go i' th' 'ut?"). Their sex preparation discussion appears to be in standard, but it is difficult to ascertain Lawrence's exact

intended representation ("Have you left your underthings off?" appears to be quite standard). Once he touches her, however, the dialect comes forth ("Eh, thar't nice!"). After intercourse, dialect dominates, as usual, even to his evaluation of her performance. As they separate, she cries out for him, leading to intercourse again, which she fully participates in this time and climaxes fully. The afterglow conversation is, as usual, in dialect. This time, however, Lady Chatterley also tries to speak the dialect:

"Tha mun come one naight ter th' cottage, afore tha goes; sholl ter?" ...
"Sholl ter?" she echoed, teasing.
He smiled.
"Ay, sholl ter?" he repeated.
"Ay!" she said, imitating the dialect sound.
"Yi!" he said.
"Yi!" she repeated.
"An slaip wi' me," he said. "It needs that. When sholt come?"
"When sholl I?" she said
"Nay," he said, "tha canna do't. When sholt come then?"
"'Appen Sunday," she said.
"'Appen a' Sunday, Ay!"
He laughed at her quickly.
"Nay, tha canna," he protested.
"Why canna I?" she said.
He laughed. Her attempts at the dialect were so ludicrous, somehow.
"Coom then tha mun go!" he said.
"Mun I," she said.
"Maun Ah!" he corrected.
"Why should I say *maun* when you said *mun*," she protested. "You're not playing fair."

It appears in this speech event that Lawrence is using dialect to describe Mellors' power over Lady Chatterley. Here, as elsewhere, their touch is accompanied by the almost immediate occurrence of dialect. As usual, sexual afterglow conversation is also in vernacular. But in this event, Lady Chatterley is so into the setting that she actually begins to speak the dialect. Though treated somewhat playfully by Lawrence, it appears to represent a significant signal on Lawrence's part that Mellors has achieved some sort of power over her.

Speech Event 8. (in Chapter XIII)

Topic: The predicament of the breakdown of Sir Clifford's motorized wheelchair.
Participants: Lady Chatterley, Sir Clifford, Mellors.
Setting: On the road.
Code: Standard English entirely. Most of the conversation is between Mellors and Sir Clifford but even the talk between Mellors and Lady Chatterley is in standard.

In this speech event Mellors assumes the role of the indoor servant, responding disinterestedly and passionlessly to Sir Clifford's insults and childish behaviour. After the worst of the behaviour, Sir Clifford speaks:

> "Do you mind pushing her home, Mellors!" he said in a cool superior tone. "I hope I have said nothing to offend you," he added in a tone of dislike.
> "Nothing at all, Sir Clifford!"

Speech Event 9. (in Chapter XIV)

Topic: Sir Clifford, Mellors' dog, Mellors' wife, divorce and marriage, types of intercourse, lesbians, their past sex life.
Participants: Lady Chatterley, Mellors, his dog, his penis.
Setting: They meet at the gate to her house, then walk to Mellors' cottage.
Code: The opening conversation, as always, is in standard. Their discussion of the wheelchair incident, his past pneumonia and Sir Clifford's lack of manhood are in SE. Mellors speaks vernacular to his dog, but discusses dogs with Lady Chatterley in standard. He talks about his wife in dialect, switching back to standard when the topic of divorce is approached. Standard English continues through the following topics (his first girlfriend, types of intercourse, lesbian women and a clinical analysis of their own sex experiences). He becomes somewhat morose and rejects the notion of their sleeping together. On his way out of the room he looks at Lady Chatterley, touches her, and erects, saying in dialect once again, "Ma little lass! Dunna let's fight! Dunna let's niver fight! I love thee an' th' touch on thee. Dunna argue wi' me. Dunna! Dunna!" As they prepare for intercourse, she admires his penis, at which point Mellors addresses it in dialect: "Ay ma lad! Tha'art thee right enough. Yi, the mun rear they head! Theer on thy own, eh? An' ta'es no count o' nobody!" The post-intercourse conversation is, again, in dialect, fading to a rather weakly represented standard English in the closing and leave-taking setting.

Speech Event 10. (in Chapter XV)

Topic: Her future trip to Venice, the colonies, Lady Chatterley's possible divorce, wealth, the Army colonel for whom Mellors once worked, tameness in men, children, hardship, life.
Participants: Lady Chatterley, Mellors, Mrs Bolton.
Setting: At the hut.
Code: Frequent switching.

At first, the topics of the forthcoming trip, the colonies, her possible divorce, wealth, the Army colonel and male tenderness are conducted in standard English. Then Lady Chatterley says: "Tell me you want a child ...". Mellors' answer begins in standard but moves quickly into dialect as he ruminates about children and the hardships of life. While he talks, she handles his genitals but fails to arouse him. As his talk moves more and more away from the hardships of a collier's life to a more philosophical discussion of the doom of all mankind, his speech becomes increasingly standard. Lady Chatterley was in no mood for such moroseness and she broke the mood by taking off her clothes and running out into the rain. This was enough for Mellors. He followed suit and caught her on the path where they had intercourse, in the driving rain. Afterward, they go back to the hut and the afterglow conversation is, as always, in dialect. After a while Lady Chatterley asks: "You don't mind, do you, that I'm going away?" With a blank expression he answered: "You do as you wish." Lawrence observes: 'And he spoke in good English.' The topics are her trip to Venice and the possibility of her divorce from Sir Clifford. Suddenly he goes outside to pick some flowers to playfully accomplish his mock wedding of his 'John Thomas' to her 'Lady Jane'. This topic sends Mellors back into dialect:

> "This is John Thomas marryin' Lady Jane," he said. "An' we mun let Constance an' Oliver go their ways ..."

His thought along with his dialect is interrupted by his sneeze, after which he talks about shirts, in general, in standard as he puts his own shirt on. He laments that perhaps his Lady Jane will meet someone else in Italy. She tells him not to say such things and he returns to dialect as he talks to 'Lady Jane' (rather than to Lady Chatterley) as though she were a separate person. Lawrence observes: 'She never knew how to answer him when he was in this condition of the vernacular.' A few sentences later, as they are walking back toward Wragby, they meet Mrs Bolton. Mellors says goodnight to both in proper standard English.

Speech Event 11. (in Chapter XVI)

Topic: Introductions, table talk, normal English, the risk involved in the affair, insulting, afterglow.
Participants: Lady Chatterley, Mellors, Hilda (Lady Chatterley's sister).
Setting: In a car, then at Mellors' cottage.
Code: Frequent switching.

Hilda and Lady Chatterley are driving. They meet Mellors, park the car and walk to the cottage. All conversation is in standard until Hilda sits in Mellors' chair. Lady Chatterley tells her to move and Mellors responds: "Sit yer still." The meal is conducted in dialect (put on rather heavily, it appears). Hilda asks why he speaks Yorkshire. He tells her it is Derby.

> "Derby, then! Why do you speak Derby? You spoke natural English at first."

"Did Ah though? An' canna Ah change if Ah'n a mind to 't? Nay, nay, let me talk Derby if it suits me. If you'n nowt against it."
"It sounds a little affected," said Hilda.
"Ay, 'appen so! An' up i' Tevershall you'd sound affected."

Hilda continued to observe Mellors, concluding, at last, that he was acting.

"Still!" she said as she took a little cheese. "It would be more natural if you spoke to us in normal English, not in vernacular."
"Would it?" he said in normal English. "Would it? Would anything that was said between you and me be quite natural ...?"

Lawrence notes here that Hilda was baffled and annoyed: 'After all, he might show that he realized he was being honoured. Instead of which with his play-acting and lordly airs, he seemed to think it was he who was conferring the honour.' Mellors continues briefly in standard until Hilda asks him if the risk of the affair is worth it. His answer is in dialect, which he stays in until Hilda insults him. His response is in calculated standard: "But you deserve what you get: to be left severely alone." Hilda stalks out leaving Lady Chatterley and Mellors alone. She initiates love-making and he moves back into dialect through intercourse and afterglow talk.

Speech Event 12. (in Chapter XVIII)

Topic: Lady Chatterley's return from Italy, what happened while she was away.
Participants: Lady Chatterley, Mellors, Sir Malcolm, Hilda.
Setting: At a hotel (first alone with Lady Chatterley, next with Sir Malcolm [her father] and last with Hilda [her sister]).
Code: Predominantly SE, with similar switches into vernacular.

The greeting behaviour, as Lady Chatterley and Mellors first meet after a long absence, is stiff and standard:

"Ah, there you are! How well you look!"

They discuss how it was for him while she was gone. He talks about how little he has to offer her in a clinical, standard fashion. He observes that the world is artificial and fickle, and that it is touch that we are all afraid of. She says, "Then hold me." He does and then begins to use dialect. Then she asks if he still loves his wife. He answers in standard and continues in it after both Sir Malcolm and Hilda come in. He cannot bear the scheme which they propose to protect her name and his in his near despair at the whole affair. "I agree to anything. The world is a raving idiot." Then he looks at Lady Chatterley and says in dialect:

"Ma lass! The world's goin' to put salt on thy tail."

CONCLUSION

Two basic questions have been addressed in this analysis of the use of code-switching by D H Lawrence in Lady Chatterley's Lover.

(1) Can an author represent a linguistically complex phenomenon such as code-switching in a realistic fashion?

Research in code-switching has clearly indicated that social conditions create the need for varying one's language use. Those who have at their disposal a range of codes, whether languages or dialects, seem to be the speakers who are advantaged. In the case of language code-switching, however, the bilingual must adjust to the language of the monolingual if any communication is to take place at all. In at least one sense of the term, the bilingual thus moves to a point of disadvantage, especially if the language being used is not his native tongue. In terms of power, he is forced to use his less powerful code by the ignorance of the monolingual.

The power-condition involved in code-switching does not always work in this way, however, especially when the codes are mutually intelligible dialects of the same language. In the case of Lady Chatterley's Lover, the vernacular and the standard are mutually comprehensible, that is, speakers of both dialects have receptive competence only in standard. Mellors has productive competence in both. The only person capable of using language variability to provide alternation, and thereby to create meaning from function itself, is the speaker who can speak (and is allowed to speak) two dialects. Only Mellors had the right to switch roles from the standard-speaking inside servant to the vernacular-speaking woodsman, since his servant roles overlap in both areas. Because of his unique background, he can also switch into a standard-speaking non-servant role. As the non-local participant, his right to switch is established. In a sense, Mellors is the only participant in the novel who can enjoy the luxury of providing meaning by the very act of code-switching. He can insult, reject, withdraw, refuse to answer, disobey, mock, hide and perform many other functions, all through the meaning offered by switching in sharp contrast with the semantic meaning of the words being used.

Linguists have little examined the power-relationship offered by this sort of control dictated by code-switching. Mellors exhibits it beautifully, in a type of linguistic fencing which parries, thrusts, and feints with the greatest artistry. He can deny his own worth and his own surreptitious intentions while at the very moment having a key made for Lady Chatterley to meet him at their trysting place. He can seduce her in standard English, then excuse his behaviour with the very vernacular which he is using to make the excuse. He can keep Lady Chatterley confused about their relationship by referring to her as 'ma lass' and 'your Ladyship' in the same speech event, at the same time permitting this very switching to be the cover for any indiscretions for which he might be guilty. And when his cover falls in danger of being exposed ("Why don't you speak in ordinary English?"), Mellors can, in his very switching, hide more truth than that which is hidden by his answer ("I thowt it *wor* ordinary").

Mellors' ultimate power created through code-switching, however, seems evident in the intercourse scenes. As noted earlier, the introduction to intercourse is in standard English in each of the eight major intercourse scenes in the novel. Mellors' conversation after intercourse is invariably in the vernacular. Lady Chatterley's language is predictably standard throughout the novel but evidence of her coming under Mellors' power seems to be apparent in intercourse scene number five (in Speech Event 7b) when, after climaxing explosively, she begins to try to speak Mellors' vernacular. He finds it charming, but somehow ludicrous, and feels obliged to play school teacher to her by correcting her inability to speak the vernacular accurately.

In answer to the first question posed by this investigation, then, we can observe that not only does Lawrence represent the linguistically complex situation of code-switching with accuracy, but that he also makes use of the basic understandings of code-switching as the critical vehicle for motivation in the novel. That is, Lawrence portrays code-switching in a manner which parallels that which might have been revealed by tape recordings of real life conversations. Even without the benefit of research on the rule-governed alternations in code-switching, Lawrence has caused Mellors' speech to vary consistently according to the major factors which subsequent research has demonstrated to be critical. Not only has Lawrence done this with amazing perception, however, he has also made use of its basic truths upon which to build the major action and assumptions of the story.

(2) Does the sociolinguist have a contribution to make to literary criticism?

It is always presumptuous of members of one academic community to claim that members of a different community will survive and prosper only if the latter learn the secrets of the former. Such is not necessarily the claim of this paper, although it seems apparent that the presumed advantages of recent efforts at disciplinary segregation have proved to be specious. Disciplines leak. It is difficult for many linguists to determine exactly what field they really belong in. Whether or not it wants to be, language is involved in philosophy, psychology, history, sociology, mathematics, anthropology, literature and education, to name only a few disciplines.

It is hoped that nothing in the current analysis is an attack on what literary critics do or do not do. The analysis of code-switching is relatively new even to sociolinguists and it is in no way fruitful to criticize a field for not doing something that is not being done elsewhere.

What this paper hopes is that certain tasks of the literary critic will be aided by recent theoretical developments in sociolinguistics. Specifically, literary critics can be helped to ascertain how effectively and how consistently an author portrays the language of his fictional characters. Recent developments in conversational analysis can provide a scientific touchstone (in well-defined contexts) for literary comparison. One type of analysis might have been to match the orthographic representation of broad Derbyshire dialect with the linguistic Atlas research which has been done in that area. In this case, we have chosen rather to make

use of a measurement point which is less concerned with the surface manifestation of language representation (the phonology and grammar) and more concerned with the meaning, particularly the sort of meaning which may or may not be consciously controlled by the author. If we had analysed the former question we could learn about how well the author knew the dialect he represented. The latter question gets at a much larger issue: how well does the author know how to make use of the uses of language?

A second type of question with which sociolinguistics can provide assistance to the literary critic is in the broad area of authorial awareness. A legitimate question often asked of authors is the extent to which they remember, mimic or otherwise write with structural integrity without apparent artistic intention. That is, how much did Lawrence really know about what he was doing with conversation in Lady Chatterley's Lover? Did he write from remembered fragments and native 'feel' or was he aware of the consequences of code-switching as he developed the dialogue?

In the case of this novel, it is clear that Lawrence must have been aware of the patterned structure of conversational rules. Not only does he structure code-switching along the patterns dictated by analysis of actual code-switching in non-fictional contexts, but he also makes authorial-voice comments on the language being used, for example:

> 'His voice on the last words had fallen into the heavy broad drag of the dialect ... perhaps also in mockery because there had been no trace of dialect before.' (Chapter V)
>
> 'He spoke cold, good English.' (Chapter XII)
>
> 'Her attempts at the dialect were so ludicrous, somehow.' (Chapter XII)
>
> 'And he spoke in good English.' (Chapter XV)
>
> 'She never knew how to answer him when he was in this condition of the vernacular.' (Chapter XV)

These and other instances of the authorial voice offer supporting evidence for the belief that Lawrence knew exactly what he was doing as he did it. Even when he put evidences of how language functions carry meaning in the words of his characters we get a clear picture of authorial knowledge and intention. The marvellous conversation about code-switching in Speech Event 11 is ample evidence of this. Hilda accuses Mellors of switching (apparently Hilda was the only character who was conscious enough of the phenomenon to be able to isolate it for conversation). Mellors responds (in vernacular) that he can switch if he wants to. To Hilda's suggestion that his Derbyshire dialect sounds affected, Mellors responds: "An' up i' Tavershall you'd sound affected." This sophisticated insight has been overlooked for decades in this country and it is no credit to either English or Linguistics departments that is is so little understood some half-century after it was written.

In summary, then, it should be repeated that recent developments in sociolinguistic analysis offer analytic assistance to the field of literary criticism. Unfortunately, the analysis performed in this paper tells us little that we did not already know about Lawrence's considerable abilities as a novelist. What it does provide is a way of capturing and discussing that ability in measurable terms, perhaps as a point for comparing that same ability in other authors but at least as a way of more concretely describing what it is that Lawrence did so well. Perhaps this is all that science can ever offer art.

REFERENCES

Ervin-Tripp, S
1964 'An analysis of the interaction of language, topic and listener', in J Gumperz and D Hymes (ed.) The Ethnography of Communication, Washington DC: American Anthropological Association, 86-102.

1972 'On sociolinguistic rules: Alternation and co-occurrence', in J Gumperz and D Hymes (ed.) Directions in Sociolinguistics, New York: Holt, Rinehart and Winston.

Fishman, J A
1972 'Domains and the relationship between micro- and macro-sociolinguistics', in J Gumperz and D Hymes (ed.) Directions in Sociolinguistics, New York: Holt, Rinehart and Winston.

Gumperz, J J
1964 'Linguistics and social interaction in two communities', in A S Dil (ed.) Language in Social Groups, Stanford: U.P., 1971.

1970 'Verbal strategies in multilingual communication', in J Alatis (ed.) Report of the 21st Annual Round Table Meeting on Languages and Linguistics, Washington DC: Georgetown U.P.

Gumperz, J J and J Blom
1971 'Social meaning in linguistic structures: Code-switching in Norway', in A S Dil (ed.) Language in Social Groups, Stanford: U.P.

Gumperz, J J and E Hernandez
1971 'Bilingualism, bidialectalism and classroom interaction, in A S Dil (ed.) Language in Social Groups, Stanford: U.P.

Hymes, D
1970 'On communicative competence', in J B Pride and J Holmes (ed.) Sociolinguistics, Harmondsworth: Penguin.

1972 'Models of the interaction on language and social life', in J Gumperz and D Hymes (ed.) Directions in Sociolinguistics, New York: Holt, Rinehart and Winston.

Labov, W
1966 The Social Stratification of English in New York City, Washington DC: Center for Applied Linguistics.

1970 'The study of language in its social context', Studium Generale 23: 30-87.

Lawrence, D H
1959 Lady Chatterley's Lover, New York: Signet.

Shuy, R W
1969 'Subjective judgements in sociolinguistic analysis', in J Alatis (ed.) Report of the 20th Annual Round Table Meeting on Languages and Linguistics, Washington DC: Georgetown U.P.

Shuy, R W, W Wolfram and W K Riley
1968 Field Techniques in an Urban Language Study, Washington DC: Center for Applied Linguistics.

Strick, G J
1974 Arabic-English Code-Switching. MA Thesis, American University of Beirut, Lebanon.

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'THEY DON'T FOOL AROUND WITH THE CREOLE MUCH, AS WITH THE SPANISH': A FAMILY CASE IN SAN IGNACIO, CAYO DISTRICT (BELIZE)

1. THE FACTS

The Sociolinguistic Survey of Multilingual Communities is a programme of research launched by Professor R B Le Page with an international team: Dr Pauline Christie (University of the West Indies, Mona, Jamaica), Dr Baudouin Jurdant (Université Louis Pasteur, Strasbourg, France), Dr Andrée Tabouret-Keller (Université Louis Pasteur, Strasbourg), Mr Douglas Taylor (Dominica and University of York, England), Mr A J Weekes (University of York, England) and with international funding: Social Science Research Council (SSRC), London; Centre National de la Recherche Scientifique (CNRS), Paris; Délégation Générale à la Recherche Scientifique et Technique (DGRST), Paris; The Ford Foundation, New York; University of York; Université Louis Pasteur, Strasbourg. The survey of Belize took two years from October 1969, approximately nine months in all being spent on fieldwork in Cayo District, one of the western inland districts of what was then British Honduras (now Belize). Cayo District lies along the Guatemalan border. Details of the historical, demographic and social setting, of the sociolinguistic problem, of the sampling procedure and linguistic analysis are given in a Preliminary Report on the survey by Le Page (1972).

Our data in this family case

On 13 February 1970 Pauline Christie interviewed a pupil of St Andrew's Primary School, in San Ignacio, the most urban sector of the district. The girl, whom we shall refer to as Irma Fairley, was then in Standard VI and her age was given as 13. The interview lasted from 10.00 to 11.15 am; it was partly recorded and the answers to a questionnaire on language use were taken down in writing. On this answer sheet Pauline wrote the following comment: 'Mother dead. Lives with father's parents (father there occasionally). Grandparents speak Spanish but father speaks Creole.'

On 10 November the same year I myself (Andrée Tabouret-Keller) went to San Ignacio to locate the house which was given on my list as Irma's address. A woman neighbour from whom I asked directions told me this was 'the house of old Mrs Fairley, Irma's grandmother'. I spent about one and a half hours with the old lady, one of her sons and Irma (with a few women neighbours coming in and out) trying to explain about the Survey and trying to get the old lady's agreement to interview Mr Fairley, Irma's father, another of the old lady's sons. This was granted at last and the interview with Mr Fairley took place the following afternoon, in the same house, from 2.00 to 4.30 pm. Irma was present and gave her age as 15 (NB: one of the facts). The interview was not recorded, but abundant notes were taken both during

the interview and later the same evening. On the interview sheet I now read the following comment: 'The large dining-sitting room is 'do minated' by an old and sick woman, in a rocking-chair. She is the grandmother, very fair (skin), and all the time I have been there (both times) her children (two sons and a daughter-in-law) asked her permission for this and that and seemed to do nothing without having told her about it and got her agreement. She also agreed that I could do the interview and she was sitting there all the time, watching carefully but didn't intervene.' I went through a psycho-sociological questionnaire with nine sections: identification of the interviewee, the father, the mother, the children; leisure and jokes, education, attitudes to foreigners, social life, the house. Each section covered several topics and contained many questions. Usually after the first few questions an informant would speak freely and the interview would resemble a conversation rather than a question-answer session. With Mr Fairley our meeting gave place to a prolonged and quite pleasant conversation.

On 9 January 1978 Robert B Le Page interviewed Irma (now married as Mrs Ramirez) at the house of her parents-in-law in San Ignacio. She was then aged either 21 or 23 and had two children. I was also present at that interview, which was recorded. The transcription used here was made by Le Page during 1979.

2. THE INFORMATION

Why a distinction between (1) The facts and (2) The information? It is a fact that when Irma was asked by Pauline Christie: 'What other language would you like to speak well?', she answered : 'Good English' but the information this fact is conveying remains hidden until one learns that people in Belize contrast 'good English' with 'Creole' which they think is 'bad English'. It is also a fact that when Irma was interviewed by Pauline in February she was 13 years old (this was checked on the school register) and that when she was interviewed by myself in November of the same year she said she was 15. Pauline's data were not then available to me and I had no reason whatever to suspect that the information was incorrect; indeed it was given both by the girl and by her father. But what information is conveyed by the discrepancy between these two 'facts'? When I interviewed Irma in November she had left school, which was consonant with being 15. When Le Page interviewed her in 1978 she told him that she left school at 14 in order to help her grandmother: 'I had to left (*sic*) school on account of my grandmother that got cripple' and that she had anyway missed school during most of her last year. We must not forget that the '15 years old' answer was given to a person (in this case myself, though my personal identity had nothing to do with it) who was linked with the school institution and who might have felt that Irma's place was in school and not as the person chiefly responsible for a whole household.

I must stress that it was usually the women we met first when we visited a house, and in many cases we met only them. But very soon I put the households into two categories: those in which the

women were very pleased to have the entertainment of a foreign lady coming to visit them and who showed their pleasure, and those in which I'm sure the entertainment was also enjoyed but where nothing, or very little, was shown of it; at any rate the presence of the foreign lady met with heavy suspicion. More than once I was asked by the latter category what I was doing so far away from my family, whereas by the former I was told how clever I was to be able to get away from my family for a while! In my very personal labels I called the former households 'Creole' and the latter 'Spanish'. The Fairley household belonged obviously to the second category but we shall see that nobody is really like a category and that behind these facile labels lie complex identities.

The information collected by Pauline Christie from the schoolgirl, Irma Fairley

Four documents are available: an answer sheet to the linguistic questionnaire filled in by Pauline, a record of the conversation Pauline had with Irma and its transcription by Douglas Taylor, and a chart of the quantities in five selected features chosen from a first detailed analysis of twenty interviews, three of them being distinctively 'Creole' (vowel nazalisation, Creole past tense forms and Creole plural forms), one being very largely due to the influence of educated usage and spelling-pronunciation which happens to coincide to some extent with 'Spanish' practice (*r*-colouration or post-vocalic *r*), and one being a marker of 'Spanish' practice also (devoicing of -Z in syllable-final position). Different techniques were used in counting these features which appear in % (Table 1). Nasalization appears as a proportion of all syllable-final nasal contexts, Creole past tense of all past tense contexts, Creole plural of all plurals that would be marked in Standard English, *r*-colouration of all places where post-vocalic *r* occurs orthographically in Standard English or Spanish, de-voicing of all voiceless syllable-final sibilants which would be voiced in R P. Each tape is divided into several sections: early conversation, later conversation, Anansi story (Anansi story-telling is an established tradition in Creole families throughout the Caribbean), other story (usually from the school stock, like 'The three little pigs') and reading (this was a prepared story and a word list to test distinctive features) (Le Page, 1972).

The record of Pauline's conversations with Irma is a rather brief one: not only was it one of the very first children's interviews done by Pauline (the fourth, in fact) and although there had been preliminary trials Pauline had not yet reached the height of her interview technique, but also the girl was not very talkative. She did not tell an Anansi story, she started to tell a version of *Cinderella which sounds like a counting game*: 'Cinderella dressed in blue went up, Cinderella dressed in yellow went up to see her grandmother; when she get up there she met a fellow', at which point the girl stopped. All her conversation is in English except for a passage in Spanish when she talks about her father, his present wife and their children. All this explains why in

Irma's case the chart of the linguistic features is rather poor: she has told neither an Anansi story nor another story, which leaves us with only three of the five expected sections (early and late conversation samples, and reading: I will give the results in this order). But the analysis of her conversation and reading features gives us enough information to place her in a category more 'Spanish' than 'Creole'. 'Creole' features are indeed very poorly represented: nasalization is almost absent (0%, 1% and 2%), the Creole past rare (17%, 8% and 6%) and the Creole plural also (0% and 12%), whereas 'Spanish' features are well represented although not very numerous (25%, 36% and 71% for *r*-colouration and 9%, 30% and 5% for devoicing). Table 1 shows her results and also the average results (Tabouret-Keller, 1979) for the group to which she belongs, of urban children who did not tell an Anansi story. Her results are always below the average except for one revealing point: during the end of the conversation (late conversation) when she apparently was more relaxed she used 30% 'devoicing' whereas the average for her group is 24%. This puts her on the 'Spanish' side, although overall no one feature was really prominent in her speech.

Feature		Conversation		reading and word list
		early	late	
Nazalisation	Irma	0	1	2
	Average	12	14	5
Creole past tense	Irma	17	8	6
	Average	30	37	11
Creole plural	Irma	0	12	
	Average	13	24	
r-colouration	Irma	25	36	71
	Average	68	64	90
Devoicing	Irma	9	30	5
	Average	22	24	13

Table 1: Linguistic features in Irma's early and late conversation and in reading, and the averages for non-Anansi-story-telling children (urban group).

The answers given by Irma to the linguistic questionnaire concerning her language use within her family are represented in Figure 1. With her grandmother she speaks 'Spanish', with her grandfather 'English' and with her father and younger sisters 'Creole'.

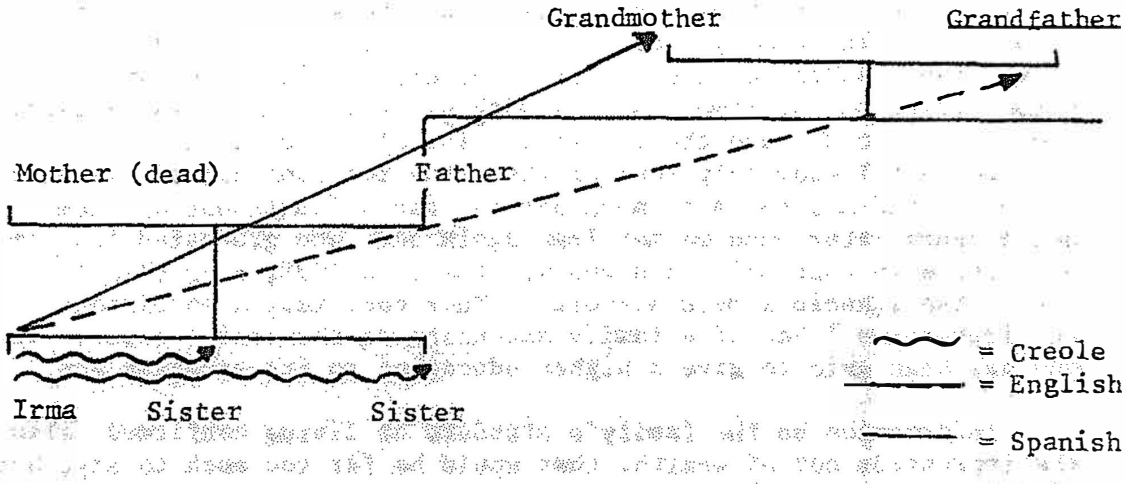

Figure 1: Irma's statements as to her language use within her family.

There is no explanation as to why she should speak English with her grandfather. Outside her family she says that she speaks Creole with her best friends (her two best friends, the names of whom she gave, are unfortunately not in our sample) when in the schoolyard or outside it and also that it is in Creole that she tells funny jokes to her schoolmates or that she quarrels with them. But, as already mentioned, it is Spanish that she feels is 'most natural' to her, whereas she thinks that English is the most useful language in British Honduras. It's 'good English' she would like to learn, she would also like to go to college in order to be able to get an office job. We have an indication on her family's standard of living: Irma says that she reads a newspaper every evening, The Reporter or The Belize Times and that the family also occasionally has a Spanish newspaper from Melchoir (the neighbouring border-town in Guatemala).

The information collected by Andrée Tabouret-Keller from Irma Fairley and her father, Mr Fairley.

The house of the Fairley family, in one of the central streets of the town of San Ignacio, is a wooden, two-storey house with a zinc roof. It stands in a clean yard with fruit trees but with no domestic animals. The house is painted outside and inside and gives an impression of fair wealth in contrast with so many houses of the town that are less large, not necessarily two-storey, rarely painted, far less well-kept, and with domestic animals all around. The house has a separate kitchen, a living-dining room where the interview took place, three bedrooms with eight beds, and a bath-room; the W C is in the yard. The house was built by Irma's grandfather to whom I am introduced and who is a very dark skinned old man. He cannot remember in which year he built the house; it was a long time ago. His first name is Joseria, he was a blacksmith and he passed on his trade and his workshop to his oldest son, also called Joseria, Irma's father. There is electricity and water in the house, the stove

burns kerosene, there is also a sewing machine and a radio in working order, unlike in so many other households where it is just a piece of furniture. The family owns a car also. Nine people live in the house: the grandfather and the grandmother, Irma's father and his three girls, one of Irma's uncles with his wife - an Indian-Spanish woman I was told - and their child. The old Joseria has several children but I know only four of them: the two sons already mentioned and one I had met the afternoon before, and a fourth one of whom I heard years later when we met Irma again and 'who graduated two times over there in Guatemala' and who was then, in 1978, a Principal in one of San Ignacio's main schools. This confirms, *a posteriori*, the impression I had of a family ascending on the social scale: this one has been able to give a higher education to its youngest son.

Information on the family's standard of living confirmed also the impression not of wealth, that would be far too much to say, but of relative ease. As several families were living together in the same house it was not possible to know how much was spent on food but Irma's father stated that his share of the household expenses was 25 B H dollars every two weeks (in 1970 BH$ 4 were worth £1 or US$ 2) and that the family's credit for food in the shop was for two weeks. Open credit in the grocer's shop is a social marker of regular income. No credit would put someone at the bottom of the social scale. A credit for two weeks confirms the continuity of the income: a *milpero* (small farmer) would have a monthly credit, limited to a certain amount, or even a credit for two months; a *chiclero's* (chicle-gatherer's) wife would have a yearly credit, also limited; whereas the Fairley family's two weeks' credit was, I gather, not limited since there was no real reason to limit it. Three meals were taken every day: breakfast at 7 am with bread, butter and fried beans, lunch at 12 with rice and beans and meat, and supper at 4 pm with bread and butter and meat. It must be noted that Irma's father used these English terms to name the meals, that the timetable for the meals and the use of the term 'supper' belong to the Creole sphere rather than to the Spanish one, and finally, that the dishes confirm more than everything else the ease of the family: in poorer families bread is replaced either by tortillas or by rice, butter is absent and meat appears with the comment 'sometimes' or 'on Sundays'. Much earlier during the interview I asked a question on average income to which Mr Fairley said he wouldn't know how to answer it as their income was not regular and that at any rate he felt that the family's standard of living was going downwards steadily; he added that it was not possible for him to save money. Poorer people do not mind answering that kind of question. In the Fairley family direct information on their state of wealth was not given, but much later during the interview when the best doctor in San Ignacio was discussed I was told that 'he was the one who looked after the grandmother', not because of any particular illness, 'just for old age'. This shows at least the concern of the family for the old lady and one can guess that there was no real difficulty paying the doctor and that there was some money on one side for that purpose.

Further insight into their economic standard is given by Mr Fairley's statement that he reads a newspaper at least once a month and also sometimes in between. Irma's statement was that she read a newspaper

every evening, but I did not know that when I visited the family and I was not able to check it. It is certainly possible that Irma's answer applied to the Fairley household whereas her father's answer applies to his second wife's household, in Central Farm, where he lives most of the time. *Vis-à-vis* education Mr Fairley expresses rather complex opinions: he feels that education is important in life and wishes for Irma to become a schoolteacher, but at the same time he doesn't express the slightest regret at keeping Irma at home or at her having missed school very often during her last year to take care of her grandmother. It was obvious that it did not occur to him that a sacrifice might be worth while for a girl to be educated. He feels that arithmetic was an important subject for Irma to learn in order for her to be able to do the family shopping properly. He had never met her schoolteachers nor does he think that teachers, generally, are particularly important members of a community. 'They are like everybody else.'

The family is Roman Catholic. All of them go to church every Sunday and the priest comes regularly to the house to visit the grandmother. Mr Fairley and his first wife, Irma's mother, were married at San Ignacio's Roman Catholic church. Mr Fairley was 37 years old when I interviewed him; he had left school at 14 to become apprenticed to his father. He has never left Belize, nor even San Ignacio except for just a few days, nor his parents' house. When asked about his main occupation he answers '*mecanico*' but later on he says that like his father he is a blacksmith and also, still later, he says that he likes his job very much (his emphasis) and doesn't think at all about getting another one. He adds that he has not always enough customers and only wishes simply for more work as a blacksmith. One should not miss the nuance in the fact that his first answer was '*mecanico*' but in all subsequent mentions his occupation was 'blacksmith'. I'm not sure about the information this nuance carries except that, being an easy way of introducing the interview, the question on jobs was asked at the very beginning of it. The term '*mecanico*' is perhaps more of a social façade than 'blacksmith', and as we will see later on 'Spanishness' is also very important as a social identity the family gives itself, one that had to be established at the outset. There might still be another element in it: there are still many horses left to shoe in Cayo District but Mr Fairley wished he had more customers and certainly even in Cayo District cars and tractors have to some extent replaced horses. As a '*mecanico*' Mr Fairley might have more customers. That he is generally preoccupied by the employment situation is shown by the fact that when a quite different topic is discussed, ie the plurality of races in the country, he says that he feels it is a problem because 'there are too many people in the place and not enough jobs'. It is not so much the plurality of races that he worries about as the number of people. And indeed he adds immediately that he feels 'it would be better to mix them (the races) and get them married to one another'. But we will see that when it comes to his own case Mr Fairley's responses are far more complex and more ambiguous than this quite simple and straightforward statement that is given from a general point of view.

He says that there are a lot of races in Cayo District; you can tell the 'Turcos' by their soft hair, the 'Caribs' by their bigger

lips, the 'Indians' by their straight long hair, the 'Waikas' who don't have much hair, the 'Coolies' who are darker and have also straight hair but 'it could be very hard to tell a "Spanish" from a "Creole", the two of them may be alike'. No mention was made at that time of the family's ethnic identity but we will see how well this last sentence applies to it.

Later in the interview Mr Fairley is asked which race he feels he belongs to. He answers with some hesitation, 'Well, Spanish. My mother is definitely Spanish ... some of us are Spanish-Creole'. This is not a clear-cut answer and it gives rather the impression of some kind of in-between position. But when talking about the languages he uses Mr Fairley is quite definite: his first language is Spanish; he learned English later, but before going to school, from his father at home. In his everyday life he speaks Spanish with his mother, English with his father and Creole with his daughters. These replies are given in the above order and I remember well that at first I thought that like many people in Cayo District Mr Fairley would prefer to say 'English' rather than or instead of 'Creole'. I was surprised when my supposition was invalidated by Mr Fairley making clearly a distinction between English, which he speaks with his father, and Creole, with his daughters. Towards the end of the interview when we discussed the neighbourhood, which is Spanish, Mr Fairley says without hesitation: 'The neighbours speak Spanish from preference but also Creole and English' - I had an opportunity then to ask Mr Fairley if he ever mixes up the languages. His answer is rather sharp: 'I'm talking one and straight that one' he says. He adds that he always keeps them separate.

In Figure 2 I have put together all the statements that were given at different times of the interview by Mr Fairley on ethnic identity, first languages and language use in his family. During my visit the day before and during the interview I had opportunities enough to observe Irma and her sister Sadia speaking Spanish to their grandmother and to one of their uncles and Creole to their father and to observe Mr Fairley speaking Spanish to his mother and Creole to his daughters. But although old Mr Fairley came in and was introduced to me, in English, he didn't say a word. I know that old Mr Fairley 'went to the States', I don't know when or for how long. We may suppose that the 'English' he taught his son was the language he had learned and spoken there and that he and his son both distinguish English from Creole. It is now possible to understand why Irma told Pauline Christie that the language she addressed her grandfather in was English; that had sounded odd in the general 'Creole-Spanish' context of her answers. Figure 2 as compared with Figure 1 shows also that Irma's statements are borne out by her father's statements. They are both corroborated by my own observations on the family's language use reported in Figure 3. This Figure illustrates how the grandmother dominates the family language situation by being the family authority and hence by focussing Spanish use; even Mr Fairley's first wife had to address her in Spanish, I was told. The grandmother also holds her family together and it is difficult to foresee what her offspring will preserve from her 'Spanishness' once she is no longer there.

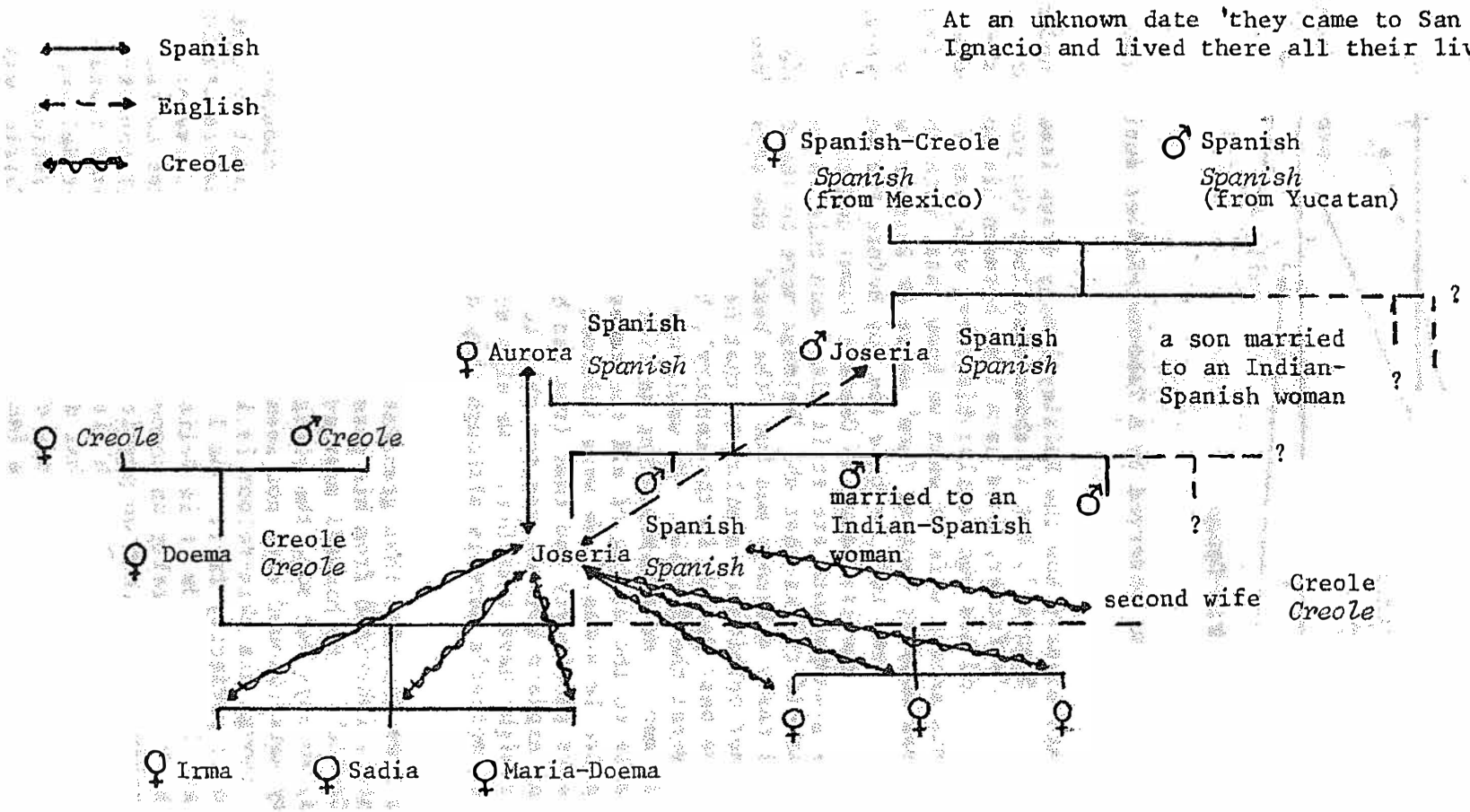

Figure 2: Statements given by Mr Fairley on ethnic identity (normal type) and first languages (Italic type) in his family and on the languages he uses (arrows with languages in Italics).

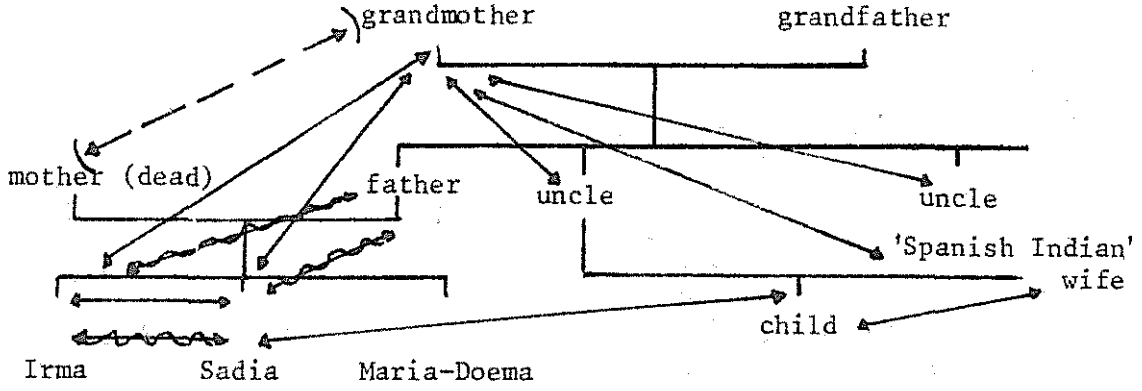

Figure 3: Language use as observed by A Tabouret-Keller during the interview.

Figure 2 represents clearly how the Fairley family draws itself under an ethnic signifier 'Spanish', but it shows also that the younger Joseria Fairley, Irma's father, acts as a link between his Spanish-origin family group and his own family: his first wife was a Creole woman and so is his second wife of whom we know that, with their four children 'she lives away at Central Farm' where Mr Fairley 'would speak Creole more'. Irma told me this the day before the interview. I asked, 'Why?' (does she live away at Central Farm) and she said 'Well, because my grandparents are still alive and this is their house'. Irma herself thinks that she 'belongs more to the Creole' and that she will marry a Creole man but 'the best', she says, 'are the Spanish'. Her opinion is similar to that her father expresses the next day: 'The best people in the country', he says, 'are the Spanish and the worst the Caribs'. He would not like his daughter to marry a Carib man. We remember that earlier on he expressed an opinion favourable to the mixing of all the races and that he himself twice married a Creole woman. We were informed that his first wife had died ten years previously and that Irma had been five years old then. This may explain why she was not able to tell an Anansi story that her mother might have told her. We also notice that she doesn't seem to meet her mother's parents although they are said to live in San Ignacio.

In Figure 2 we see also that grandfather Joseria keeps the 'Spanish' identity inherited from his parents even though his mother was 'Spanish Creole'. He himself married Aurora, a 'definitely Spanish' woman, so says her son, the younger Joseria. That the old Joseria is very dark-skinned does not seem to affect his 'Spanish' identity, even though his skin-colour is in complete contrast to that of his wife who is perfectly fair. As I have shown elsewhere ethnic identity and reactions towards it in Belize depend on positive or negative values or more generally on connotations, attached to each ethnic denomination, rather than on physical attributes (Tabouret-Keller, 1975), and also 'to be Spanish' is an over-estimated feature particularly in women and in matriarchal descent (Tabouret-Keller, 1976). The data confirm these results, obtained on statistical bases. One might conjecture that the fact that old Mr Fairley was

a representative of 'Englishness' within the family (by virtue of his U S residence) compensated in part for his dark skin and for some of his 'Creole' behaviour, but this is pure conjecture. Whatever is spoken under the name of English within the family we can be sure that in addition to 'being Spanish', 'speaking English' is also an important identification mark for the behaviour of the family members. For example, Mr Fairley expresses the opinion that a foreigner arriving in Belize should learn English first and then Spanish and then, eventually, Creole. He says also that Irma had learned to speak English at home from himself before going to school. We know already that this had been his own case when a child: he had learned English at home from his father (whatever *English* may mean).

The information collected by Robert B Le Page from Mrs Irma Ramirez, born Fairley.

The interview took place in the house of Irma Fairley's in-laws (see appendix 1). She seems to have had many regrets at having had to leave school: 'I was the only one that could help my grandmother', she said. Although the grandfather was planning to send her to take some typing lessons this never came about, and Irma had to look after her uncles, her two sisters and apparently a child (a small boy) of one of her uncles - eight people in all.

Mrs Irma Ramirez readily gives an account of language use in her family as she remembers it from the time when she was a girl. This is summarized in Figure 4 for comparison with Figure 3. It is noticeable that she leaves her grandmother out of her account (she had mentioned her as the cause of her having to leave school, as stressed above). It is also noticeable that the grandfather is no longer for her the representative of English but that with his sons he forms a group with whom one had to use Spanish. That communication was in Creole between Irma's father and his girls is confirmed and the reason for that is given: 'It was on account of my mother, my mother was Creole'. Perhaps in being consistent in speaking Creole with his girls Mr Fairley expresses faithfulness towards his wife's identity and also stresses that for such a matter priority is given by him to the mother of his children and not to his own mother.

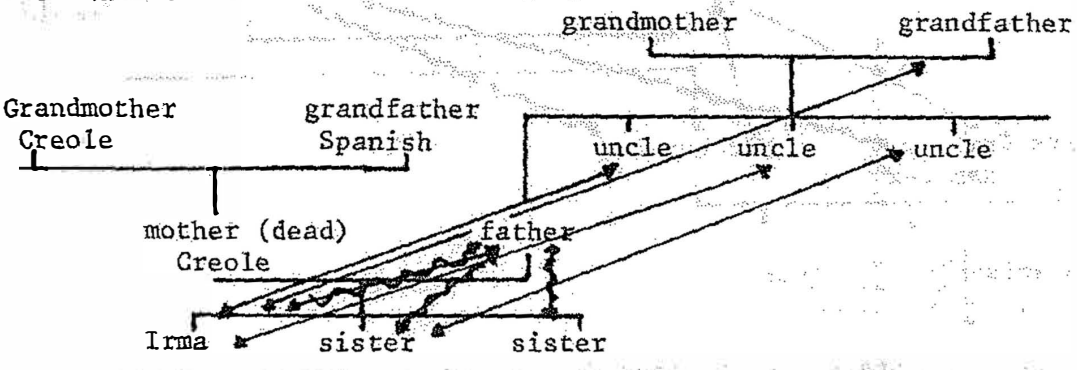

Figure 4: Statements by Irma Fairley as to language use in her family when she was a child and to the identity of her mother's parents.

From Mrs Irma Ramirez we learn also that her mother's parents, of whom we knew only that their first language was Creole, were a 'Creole' woman and a 'Spanish' man. Mrs Irma Ramirez confirms the racial identities and language use that her father had given for the neighbourhood (Table 2) but she substitutes *mixed* for *Spanish* for the racial identity of the family. We must of course remember that during Mr Fairley's interview the 'Spanish' grandmother was watching and that may have influenced Mr Fairley's reply. We see also in Table 2 that Mrs Ramirez leaves English out of the languages used within the family and that she gives equal weight to Spanish and Creole.

Social group	Identities	Mr Fairley 1970	Mrs Ramirez 1978
FAMILY	race	SPANISH	MIXED MESTIZO
	languages	SPANISH also English and Creole	SPANISH CREOLE
NEIGHBOURS	race	SPANISH	SPANISH
	languages	SPANISH Creole	SPANISH Creole

Table 2: Race and language identities as stated by Mr Fairley in 1970 and by his daughter in 1978.

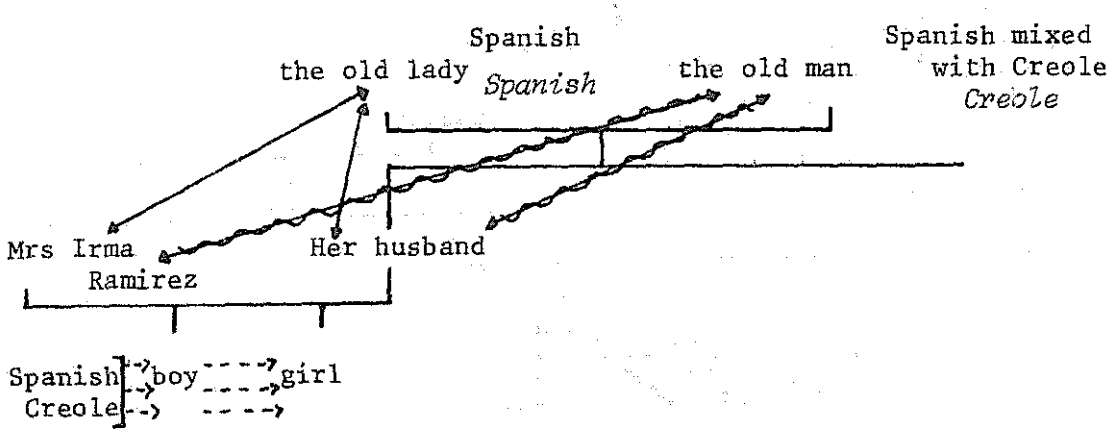

Figure 5: Statements by Irma Fairley as to identities and to language (in Italics) use in her husband's family.

Mrs Irma Ramirez described her husband's family as Spanish but she specifies: 'We would say Spanish but I guess it's more a mix with Spanish than with Creole'. It seems that Mrs Ramirez is more open than her fathers' generation had been to the idea of mixture and to degrees of mixing: 'more Creole than Spanish', is the way in which Mrs Ramirez identifies her mother, 'more Spanish than Creole' her husband's family, 'Spanish mix with Creole' her father-in-law. She supposes that her father would have said that his family is mixed. 'We call it mixed but it's mestizo I guess we are mixed with different types of races'. She seems to be certain that her father would have called their neighbourhood Spanish but here again she introduces nuances: 'most of them talk Spanish, all of them there around talk Spanish .... They talk Creole but it's not something they know how to speak it. They don't fool around with the Creole much, as with the Spanish'.

It is interesting to note that the Ramirez family, into which Irma Fairley married, duplicates somehow the combinations of the Fairley family: in both cases we see 'the old lady', that is, Irma's grandmother in the Fairley family, her own children's grandmother in the Ramirez family, focussing both Spanish identity and the use of Spanish. But Mrs Irma Ramirez claims that 'most of the time we talk Creole, we the young ones', and from that we guess that with her husband Mrs Irma Ramirez speaks Creole although we did not ask about it specifically.

3. CONCLUDING REMARKS

First of all, this family case study of the evolution over seven years of language behaviour and of opinions on language behaviour allows us to check that the different data available to us, obtained by different methods (interviews, linguistic descriptions, sociological data, various statistical analysis, etc.) relate well to one another. They are coherent within one general framework:

1. They illustrate the complexity of the linguistic network which one individual child has to explore and within which it has to locate its one place by adopting various identification marks, linguistic and more generally cultural.
2. They confirm the general impression which people in Belize but also all members of our team have formed that Creole is spreading. In this case we see in detail how the use of 'Creole' overtakes the use of 'Spanish' in the younger generation.

This case study illustrates also once again (Tabouret-Keller, 1975, 1976) how language behaviour and family and ethnic identities do not necessarilly coincide. Physical attributes (colour and texture of hair, skin colour, form of lips and nose, etc.) are being applied to categorize people outside of the family but within the family other attributes, largely or even only to be found within language itself like 'being Spanish' or 'being Creole', are sought for and their incidence seems to be much more powerful than are the mere physical marks.

Between our first Survey (1970-1971) and our follow-up Survey (1978) British Honduras moved on to become a new State called Belize.

We still have to inquire into the consequences of this political move on people's identities and loyalties. The present case study shows that the younger generation accepts more easily mixing of 'races', a mixed origin, and even the status of 'being mixed' than does the older generation. The ground is certainly prepared for an education system that would include both Creole -- if only as the medium of instruction for small children -- and also Spanish as a target language next to English. Young parents like Mrs Irma Ramirez seem ready for it.

APPENDIX I

Irma Ramirez who was Irma Fairley when we interviewed her earlier, and is now living in the same house as the Ramirez family.

LeP Can you tell us all about your, what you did at school, you know and what you've done since? What subjects did you do at school?
IR Well, at school I did Arithmetic, English, em, Science, Geography a lot of subjects all of them were like,like the arithmetic. But then we ... em I had to left school on account of my grandmother that got cripple and I had to lef' the school. An' ... I left the school at the age of fourteen, I took er, the primary examination but due to I was missing too much school, about three months school. It ... they didn't erm, I didn't ... I failed it. Em, I decided to take care of my grandmother after all, they didn't want me to continue the schooling, I want to continue but, I had to, I was the only one that could have help my grandmother.
LeP Where was your grandmother living?
IR In San Ignacio.
LeP How many children were at home then? How many children were there at home?
IR Erm, about eight of us ... were at home. I had to take charge of all of us, to do the homework myself. I didn't have any other help, I was the eldest.
LeP Eight children?
IR Yes. I was the eldest. I had my uncles along with me an' I had to take care of all of them.
LeP It's a big job. What did you have to do for them?
IR Had to cook, make lunch because one of them were working and he had to go to work four oclock in the morning. Had to get up soon and make lunch. Then I had to wash, clean out the house, get my sisters ready to school. And I had a little boy who I was, who I took care of till I come out of school. I took him over from my uncle. I had a lot of work that time. To work in the house. The old, my grandfather was planning to sent me to take some typing lesson but the same time he made up his mind the erm, person went away, I couldn't have taken it again.
LeP So then what happened?
IR I continue working until I got a boyfriend and we got married. Now I have two children, a boy and a girl. The little boy is two years and the little girl is 11 months, which will be nex(t) week. Thursday she will completing, be completing her year.
LeP How many children will you have altogether do you think?
IR I don't know how many.
LeP Do you plan for a large family or ...
IR No, I don't plan for a large family.
LeP How many do you think would be nice?
IR I think four or six.
LeP Is your husband, er... your, yourself, you're related to Gilberto Fairley are you?
IR Yes, er, that's my uncle.
LeP That's your uncle.
IR That's my uncle.

LeP What's he doing now?
IR He is the principal of the Western em, Western High College.
LeP Oh is he? Because he went over to Guatemala for training, didn't he?
IR Yes, he went over to Guatemala. He graduated over there. Two times, I guess, yes, two times he graduated over there. Now he came over this side, em, as the principal of the Western High College. Now he got married ... las(t) year.
LeP Who is he married to?
IR To Assoluta Bejoz.
LeP If I had asked your grandfather, when you were living there what sort of people lived around where you lived, what would he have told me? If you can think back seven years, you see, to your house where you were living and I, and I had said to your father 'What sort of race are the people living just round you here?' What would he have said, do you think?
IR Spanish, he would have said.
LeP He would have said Sp... only Spanish?
IR Spanish. Spanish people.
LeP No other?
IR Most of them talk Spanish, all of them around there talk Spanish. They don't, they talk Creole but it's not something that they know, to, how to erm, speak it. They don't erm, they (?didn't) fool around with the Creole much, as with the Spanish. All of us home used to erm, talk Spanish with our grandfather and with our father, we talk Creole. And all with the ... all the other erm, uncles we talk Spanish.
LeP Why do you talk Creole with your father?
IR He talk erm, talk to us in Creole, so I, I think we grew up with that, too.
LeP Why did he talk in Creole to you?
IR I don't know why maybe it was on account of my mother, my mother was a Creole an' she used to talk most Creole than Spanish. She is a bit mixed with Spanish and Creole but she talk more erm, in more Creole.
LeP Where did she come from?
IR Right from San Ignacio.
LeP But what were her parents?
IR What were he...?
LeP What were her parents?
IR Em... my grandmother is Creole, and my grandfather is erm,Spanish.
LeP Spanish from Mexico, or from where?
IR I don't know from ... Spanish from where. (? He takes ...) a Spanish. (Goat bleating here)
LeP So if I had said to your father, seven years ago, 'What do you reckon your family is?' What would he have said?
IR He would 've said erm, mixed, we ... we call it mixed but it's mestizo I guess, you see but we call it mix, we are mixed with different types of races.
LeP And mixed Spanish and Creole?
IR Yes, mixed Spanish and Creole.
LeP But now you've married into the Ramirez family? What sort of family is that?
IR Spanish too.
LeP It's a Spanish family?

IR Spanish family. Well, we would say Spanish but I guess it's more a mix with Spanish than with Creole, Bejoz, because they have some ... Creole families.
LeP Do they?
IR Yes, they have Creole families, part of the father, Mr Algueno.
LeP He's Spanish is he?
IR He's Spanish mix with Creole.
LeP And what about the mother?
IR She is Spanish.
LeP Pure Spanish?
IR Pure Spanish. Yes, pure Spanish.
LeP And what do they talk in this house most of the time?
IR Well we t ... most of the time we talk Creole, we the young ones but when it comes to the old lady we have to talk to her in Spanish. With the old man we talk in Creole, but only with her we talk in Spanish. 'Cos she understand it but she don't like to speak it to us.
LeP Do you think you'll ever go back to college and do any more?
IR No, I wish I could get back to college.
LeP Do you?
IR I wish.
LeP Why can't you?
IR Well it certainly be now like how I have two kids, that would keep me back, you know, but ... by my part, an' husban' part, yes.
LeP He would like you to go to college?
IR Yes, he would like me to. Maybe because here in this place we don't have ... that we can, we can get married and we can continue studying and that I would have take ... in one of the course.
LeP Could you go up to Belize and study there?
IR Yes, I can go. I have families over there.
LeP Well, why don't you? (No answer) What does you husband do?
IR He is, he does, he da ... is a labourer, he does any kind of work. Anything, he does. Right now, right, he is on erm, driving tractors, pulling the tobaccos, making ridge an'... to plant new tobaccos and so.
LeP What sort of language does he use with the people he works with?
IR English. Because that's an Englishman.
LeP It's an Englishman he works for is it?
IR Yes ... he was, the man was working in Africa.
LeP Who's that?
IR Danos. He was working in Africa.
LeP And what will you bring your children up to speak?
IR Both language, Spanish an' Creole.
LeP You think it's necessary for them to know Spanish?
IR Yes, I think it's very ... important for them to know Spanish because one of these days maybe there will be an opportunity for them to go out and an' they need the ... sometimes you have opportunities that you should, erm, learn to speak two languages at least and maybe Spanish, English would help them.
LeP Yes. Andree, did you want to ask any?
AT-K No I think that's fine.
LeP That's ... what's ... what exciting things have you done recently? Have you been to the pictures?

IR Yes. Well, the last picture I saw which was exciting, was the Exorcist.
LeP Ooh, tell me about that, I haven't seen it yet.
IR I don't like that picture. (Laughs)
LeP You tell me about it. What happened?
IR The little girl turn into, she was turning em, into the devil, or the devil was having some kind of relation with her. And she was, she looked very ugly. And it was an excited picture.
LeP Were you frightened?
IR Ooh, frighten. I still remember now, when it gets dark an' I found myself alone that, that picture come right into my mind and I get 'fraid.
LeP What do you do?
IR Just pray. Pray then.
LeP Can you tell me any more about it, what happened in it?
IR The ... the mother, they had to try the mother to the ... for the em, little girl, and they called the priest and so on to help the little girl and when they went and sprinkled the holy water on her, her skin start to burst. Her skin burst and she went, ... when they were praying she went way up in the ceiling, the house ... lif' her up an(d) it came down back and then they erm, the spirit got into the Father and the Father erm, threw himself from the ... erm, through the window, and he drop downstairs an' fell dead, and his head turn right around to his back, his face was towards his back and he ... back of his head towards his erm, front.
LeP You're giving me nightmares now! Well, thank you very much, I think that's all....

REFERENCES

Le Page, R B
1972 'Preliminary report on the sociolinguistic survey of multilingual communities, part 1: Survey of Cayo District, British Honduras', Language in Society 1: 155-72.

Le Page, R B, P Christie, B Jurdant, A J Weekes and A Tabouret-Keller
1974 'Further report on the sociolinguistic survey of multilingual communities: Survey of Cayo District, British Honduras', Language in Society, 3: 1-32.

Tabouret-Keller, A
1975 'Un champ sémantique: les noms d'appartenance raciale au Honduras Britannique', La Linguistique 11.1: 123-33.

1976 'Ethnic names and group identity in British Honduras', Rassegna Italiana di Linguistica Applicata 8.2-3: 191-201.

1979 'Le jeu varié de l'expression linguistique des enfants. Le cas du district de Cayo à Belize', Linguistique fonctionnelle. Débats et perspectives, Paris: PUF, 57-75.

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ACTS OF CONFLICTING IDENTITY: A SOCIOLINGUISTIC LOOK AT BRITISH POP SONGS

Anyone with an interest in British rock and pop songs will have observed that there are 'rules' concerning the way in which the words of these songs are pronounced.[1] The label 'tendencies' might be more appropriate than 'rules' in some instances, but in any case it is clear that singers of this form of music employ different accents when singing from when they are speaking, and that deviations from their spoken accents are of a particular and relatively constrained type. This phenomenon of employing a modified pronunciation seems to have been current in popular music for some decades, probably since the 1920s, and has involved a number of different genres, including jazz, 'crooning', and so on. It became, however, especially widespread and noticeable in the late 1950s with the advent of rock-and-roll and the pop music revolution.

Analysis of the pronunciation used by British pop singers at around that time, and subsequently, reveals the following rules and tendencies. (We ignore features such as the pronunciation of *-ing* as [In] which are typical of most informal styles.)

(i) The pronunciation of intervocalic /t/ in words like *better* as [t] or [ʔ], which are the pronunciations used by most British speakers, is generally not permitted. In pop-singing, a pronunciation of the type [ɾ̝] (a voiced alveolar flap of some kind) has to be employed.

(ii) It is not permitted to pronounce words such as *dance, last* with the /a:/ that is normal in speech in south-eastern England. Instead they are pronounced with the /æ/ of *cat* (as in the north of England, although the realization is usually [æ] rather than the northern [a]). In addition, words such as *half* and *can't*, which are pronounced with /a:/ by most of those English speakers, of north and south, who have the /æ/-/a:/ distinction, must also be pronounced with /æ/. Thus:

	cat	*dance*	*half*
S.E. England	/æ/=[æ]	/a:/	/a:/
N. England	/æ/=[a]	/æ/	/a:/
pop-song style	/æ/=[æ]	/æ/	/æ/

(iii) Words such as *girl, more* tend to be pronounced with an /r/ even by those English English speakers (the majority) who do not have non-prevocalic /r/ in their speech.

(iv) Words such as *life, my* tend to be sung with a vowel of the type [a·] although they are normally pronounced by a majority of British speakers with a diphthong of the type [aI~ɑI~ʌI].

(v) Words such as *love, done* tend to be pronounced with a vowel of the type [ə·] rather than with the [ɐ̈~ɐ] typical of the south of England or the [ʊ~ɣ] typical of the north.

(vi) Words such as *body, top* may be pronounced with unrounded [a] instead of the more usual British [ɒ]

There are, of course, British varieties which do have these features: most English northern and midland varieties have /æ/ in *path* and a few have /æ/ in *can't* while some south-western English and most Scottish varieties have no /æ/-/a:/ distinction and may have an [æ]-like vowel in all these words; many south-western English varieties have [d] in *better*; many western, north-western and Scottish (and other) varieties do have non-prevocalic /r/; some north-western accents do have /aI/ as [à·]; many midland and south-western varieties have [ə], although not usually [ə·], in *love*; and some East Anglian and south-western varieties have [ɑ] in *body*. The point is, however, that no single British variety has all these features, and the vast majority of singers who use these forms when singing do not do so when speaking. There can be no doubt that singers are modifying their linguistic behaviour for the purposes of singing.

EXPLANATIONS FOR LINGUISTIC MODIFICATION

An interesting question, therefore, is: why do singers modify their pronunciation in this way? One theory that attempts to deal with language modification of this kind is the socio-psychological *accommodation theory* of Giles (see Giles and Smith, 1979). This, briefly, attempts to explain temporary or long-term adjustments in pronunciation and other aspects of linguistic behaviour in terms of a drive to approximate one's language to that of one's interlocutors, if they are regarded as socially desirable and/or if the speaker wishes to identify with then and/or demonstrate good will towards them. This may often take the form of reducing the frequency of socially stigmatized linguistic forms in the presence of speakers of higher prestige varieties. The theory also allows for the opposite effect: the distancing of one's language from that of speakers one wishes to disassociate oneself from, or in order to assert one's own identity.

Accommodation theory does go some way towards accounting for the phenomenon of pop-song pronunciation. It is clearly not sufficient, however, since it applies only to conversational situations. And we cannot assume that pop musicians adjust their pronunciation in order to make it resemble more closely that of their intended audience, since what actually happens is in many respects the reverse.

Another, less elaborate way to look at this problem is simply to discuss it in terms of the sociolinguistic notion of 'appropriateness'. As is well known, different situations, different topics, different genres require different linguistic styles and registers. The singing of pop music in this way, it could be argued, is no different from vicars preaching in the register appropriate to Church of England sermons, or B.B.C. newsreaders employing the variety appropriate for

the reading of the news. Certainly 'appropriateness' is obviously a relevant factor here. But, equally obviously, it is not on its own enough to provide an explanation for why it is this type of singing which is regulated in this way, nor why it is characterized by this particular set of pronunciation rules and tendencies rather than some other.

A more helpful approach, it emerges, is the theory of linguistic behaviour developed by Le Page. Indeed, the pronunciation of pop-song lyrics provides a useful site for a microstudy which exemplifies many aspects of Le Page's thinking. This theory, expounded by Le Page in a number of writings (see Le Page, 1968, 1975, 1978; Le Page *et al*, 1974) seeks to demonstrate a general motive for speakers' linguistic behaviour in terms of attempts to 'resemble as closely as possible those of the group or groups with which from time to time we [speakers] wish to identify'. (There are, obviously, similarities with accommodation theory here, but, as we shall see below, Le Page's approach is both more detailed and more helpful.)

In Le Page's terms, British pop singers are attempting to modify their pronunciation in the direction of that of a particular group with which they wish to identify - from time to time (ie when they are singing). This group, moreover, can clearly, if somewhat loosely, be characterized by the general label 'Americans': The six pronunciation rules and tendencies outlined above are all found in American accents, and are stereotypically associated by the British with American pronunciation. (If there were any doubt about the identity of the model group, this could be confirmed by reference to the words of pop songs themselves which, even if written by British composers for British consumption, tend to include forms such as *guy* (=*chap, bloke*), *call* (*phone*) etc, which are still Americanisms for many British speakers today, and were certainly Americanisms in the 1950s and 1960s.

The next question therefore is: why should singers attempt to imitate what they consider to be an American accent? The reason for this is fairly transparent, even if somewhat intuitively arrived at, and without empirical verification. Most genres of 20th century popular music, in the western world and in some cases beyond, are (Afro-) American in origin. Americans have dominated the field, and cultural domination leads to imitation: it is appropriate to sound like an American when performing in what is predominantly an American activity; and one attempts to model one's singing style on that of those who do it best and who one admires most.

There are parallels here with other musical genres: British folk-singers often adopt quasi-rural accents; and singers of songs in the reggae style often attempt Jamaican accents. We also have to note that, in many European countries at least, it is not a particular variety of English but simply English itself that has become associated with pop music: at one time, for example, many West German pop groups had English-language names and sang songs, written by Germans, in English - a phenomenon which cannot be entirely explained by a desire to conquer international markets. (It is difficult to think of precise parallels of cultural domination in fields other than music, but one candidate might be the quasi-English accents adopted by

American Shakespearean actors even when acting in plays set in, for example, Verona.)

CONSTRAINTS ON LINGUISTIC MODIFICATION

British pop singers, then, are aiming at an American pronunciation. The end-product of this language modification is, however, by no means entirely successful. One obvious measure of their lack of success is that many American listeners are utterly unaware that that is what British singers are trying to do. The results of this modification, too, are complex and subject to change.

This also can be accounted for in Le Page's theory. The theory provides for the fact that, in modifying our linguistic behaviour, our performance as speakers 'is constrained by considerations which fall under one or another of four riders to the general hypothesis'. We will discuss these riders in turn. The first of Le Page's riders is that our modification of our linguistic behaviour is constrained by:

> '(i) the extent to which we are able to identify our model group' (Le Page, 1978).

We have already seen that British pop singers have, presumably without giving it too much conscious thought, successfully identified their model group as 'Americans', and that they attempt, again presumably for the most part below the level of conscious awareness, to model their language behaviour on that of American singers. More detailed study, however, suggests that they have not been especially successful in identifying *exactly which* Americans it is they are trying to model their behaviour on. Comparison with the linguistic behaviour of American pop singers is instructive at this point, for there is a strong tendency for them, too, to modify their pronunciation when singing. Modifications made by American singers include (a) the use of the monophthong [a·] in *life* etc by singers who have diphthongs in their speech (ie the same modification made by British singers); and (b) the *omission* of non-prevocalic /r/ in *car* etc by singers who are normally *r*-ful in their speech. A good example is provided by Bob Dylan, who is from Minnesota, in the American Mid-West, and who has /ai/=[ai] and non-prevocalic /r/ in his speech. His singing style incorporates frequent use of [a·] and *r*-loss:

> You may be an ambassador [æm'bæsədə]
>
> To England or [ə] France
>
> You may like [la·k] to gamble
>
> You might [ma·t] like [la·k] to dance
>
> ('Gotta serve somebody', *Slow Train Coming*, 1979).

These two features suggest that the model group whose pronunciation is being aimed at by American singers consists of Southern and/or Black singers, since the combination of [a·] =/ai/ and *r*-lessness is most typical of the varieties spoken by these groups. The reason for this is again clear: it is in the American South and/or amongst

Blacks that many types of popular music have their origins. (This is most obviously true of jazz, rhythm-and-blues, and rock-and-roll.) Cultural domination therefore causes singers with White non-Southern accents to modify their pronunciation when singing.[2]

This leads us to suppose that it is these same groups whose accents British singers too are aiming at, since they also, as we have already seen, have the [a·]=/ai/ feature in their singing styles. This supposition is strengthened, first, by the fact that other features of Southern and Black pronunciation, in addition to [a·], can be heard to occur from time to time in British pop songs:

(a) pronunciations such as *boring* [bourIn], and the occasional rhyming of words such as *more* with words such as *go*;

(b) the occasional inhibition of the pronunciation of linking /r/, as in *four o'clock*, without the insertion of a pause or glottal stop;

(c) the pronunciation of /I/ as [ɛ~ë] before /n/, /ŋ/, as in *thing* [θëŋ], in imitation of the Southern and Black merger of /I/ and /ɛ/ before nasals.

Secondly, it is also confirmed by the occurrence, even in songs written by British pop musicians, of grammatical features associated with Southern and Black dialects:

(a) *copula deletion*

'He livin' there still' (Beatles *White Album*)

'My woman she gone' (Dire Straits *Dire Straits*)

(b) *3rd-person -s absence*

'She make me cry' (Stranglers *Rattus Norvegicus*)

'Here come old flat top' (Beatles *Abbey Road*)

(c) *negativized auxiliary pre-position*

'Ain't nothin' new in my life today' (Supertramp *Breakfast in America*)

If this is so, then in one respect the British singers are in error. Blacks and Southerners are typically *r*-less. This fact is, as we have seen, recognised by many American singers, who (variably) delete non-prevocalic /r/ when singing even though their speech is *r*-ful. British singers, on the other hand, do the reverse: they insert non-prevocalic /r/ in singing even though their speech is *r*-less. (This contrast between British and American singers was particularly marked in the late 1950s when singers such as Cliff Richard, who were to a considerable extent imitators of Elvis Presley, attempted the pronunciation of non-prevocalic /r/ in their songs, even though Elvis himself was for the most part *r*-less.) We do not deny, of course, that some Southern varieties are *r*-ful. Nevertheless, it is possible

to argue that, in their performance of pop songs, British singers exhibit a certain lack of success in identifying who their model group is. The two perceptions - that the model group consists of (a) Americans in general, and (b) Southerners and/or Blacks in particular - conflict when it comes to non-prevocalic /r/, since in the case of the particular model group being aimed at, the stereotype that 'Americans are *r*-ful' is inaccurate.

Le Page's second rider also turns out to be relevant for our study of the linguistic behaviour of British pop singers. This rider states that our linguistic behaviour is constrained by:

> '(ii) the extent to which we have sufficient access to [the model groups] and sufficient analytical ability to work out the rules of their behaviour.'

There is evidence that British singers' analytical abilities are in fact sometimes not sufficient. This is provided, again, by the case of non-prevocalic /r/. Like, for example, many British actors attempting to imitate *r*-ful rural accents, British pop singers often insert non-prevocalic /r/s where they do not belong. This is a form of hypercorrection. Singers know that, in order to 'sound like an American', one has to insert an /r/ after the vowels /a:/ as in *cart*, /ɔ:/ as in *fort*; /ɜ:/ as in *bird*; /Iə/ as in *beard*; /ɛə/ as in *bared*; and /ə/ as in *letters*. The problem is that some singers have not mastered the principle behind where this should and should not be done: they are liable to insert an /r/ after the above vowels even where an /r/ is not required, as in *calm, taught, ideas, Americas* (*bird* and *bared*-type words are not a problem, since these vowels occur only before a potential /r/). The correct strategy to follow (except in the case of the word *colonel* - which does not occur too often in pop songs) is to use the orthography, which always has *r* where an /r/ is required. Phonologists must find it interesting, however, that, in spite of the sure guide that orthography provides, mistakes do occur - strong counterevidence to the claim that all *r*-less accents have underlying /r/ plus an *r*-deletion rule. Clearly, speakers who make mistakes of this type do not have underlying /r/. (It is possible (see Knowles, 1978) that we should distinguish between two types of hypercorrection: that due to an inability to perform *r*-insertion correctly in the heat of the moment, although the speaker actually knows how it *ought* to be done; and that due simply to ignorance of where it should be done. Ignorance, however, seems to be the main factor, since in some cases hyper-American /r/s are repeated again and again in the course of the same song.) Examples of complete lack of success in analysing the model accent correctly include:

(i) Cliff Richard, 'Bachelor Boy' (1961):

'You'll be *a bachelor boy* ...' /ər bæčələr bɔi/ - repeated many times

(ii) Kinks, 'Sunny Afternoon' (1966):

'...*Ma and Pa*' /ma:r ən pa:r/

(iii) Paul McCartney, 'Till there was you' on *With The Beatles* (1963):

'I never *saw them* at all' /sɔ:r ðɛm/.

Le Page's rider indicates that sufficient contact with the model group might diminish this kind of mistake, but even close personal contact with speakers of American English seems in this instance to have relatively little corrective influence. One explanation for this may be that Americans are unlikely to offer any overt, accurate correction of hyper-American /r/ because they (like Scots) are used to the idea that 'English people put in *r*'s in the wrong place'. They are, it appears, unable to distinguish between the *intrusive /r/*, which occurs only before a following vowel (as in *Ma* in the above example (ii) from The Kinks), and is a normal part of everyday pronunciation in many Australasian, English (and indeed some American) English accents, and *hyper-American /r/*, as in *Pa* in the Kinks' song, which occurs before consonants and pauses, and is found only in singing. Indeed, even well-known American phonologists have been heard to assert that 'British speakers say *America* /əˈmɛrɪkər/'. (There are in fact some English English accents which do genuinely have /r/ in *last* etc and/or *vanilla* etc (see Orton *et al*, 1978; Wells, forthcoming), but none of the pop singers guilty of hyper-American /r/ speaks one of these varieties, so far as I know.)

Le Page's fourth rider (we shall return to the third shortly) is:

'(iv) our ability to modify our behaviour (probably lessening as we get older)'.

We have already touched on this point in our discussion of hypercorrection: it is possible that imperfection in imitation is due to lack of ability. We can further demonstrate the validity of this rider by pointing to the fact that most of the modifications British singers make in their pronunciation are variable, irregular and inconsistent. We can assume, for instance, that many singers would pronounce all non-prevocalic /r/s if they could. It is simply that, in the flow of the song, they are not consistently able to do so. It is also of interest to observe that some phonological environments cause more difficulty than others. Most difficult, apparently, is the insertion of non-prevocalic /r/ in an unstressed syllable before a following consonant, as in *better man*. Here, fewest /r/s are pronounced by British singers. Correspondingly, it is in exactly the same environment that *fewest* /r/s are *deleted* by American singers.

The observation that non-prevocalic /r/ is only spasmodically inserted in singing can be confirmed quite simply by counting. For instance, in spite of a strong impression, to British listeners, that they are in this respect successfully imitating Americans, The Beatles, on their first British LP, *Please Please Me* (1963), manage to pronounce only 47% of potential non-prevocalic /r/s.

CHANGING PATTERNS OF LINGUISTIC MODIFICATION

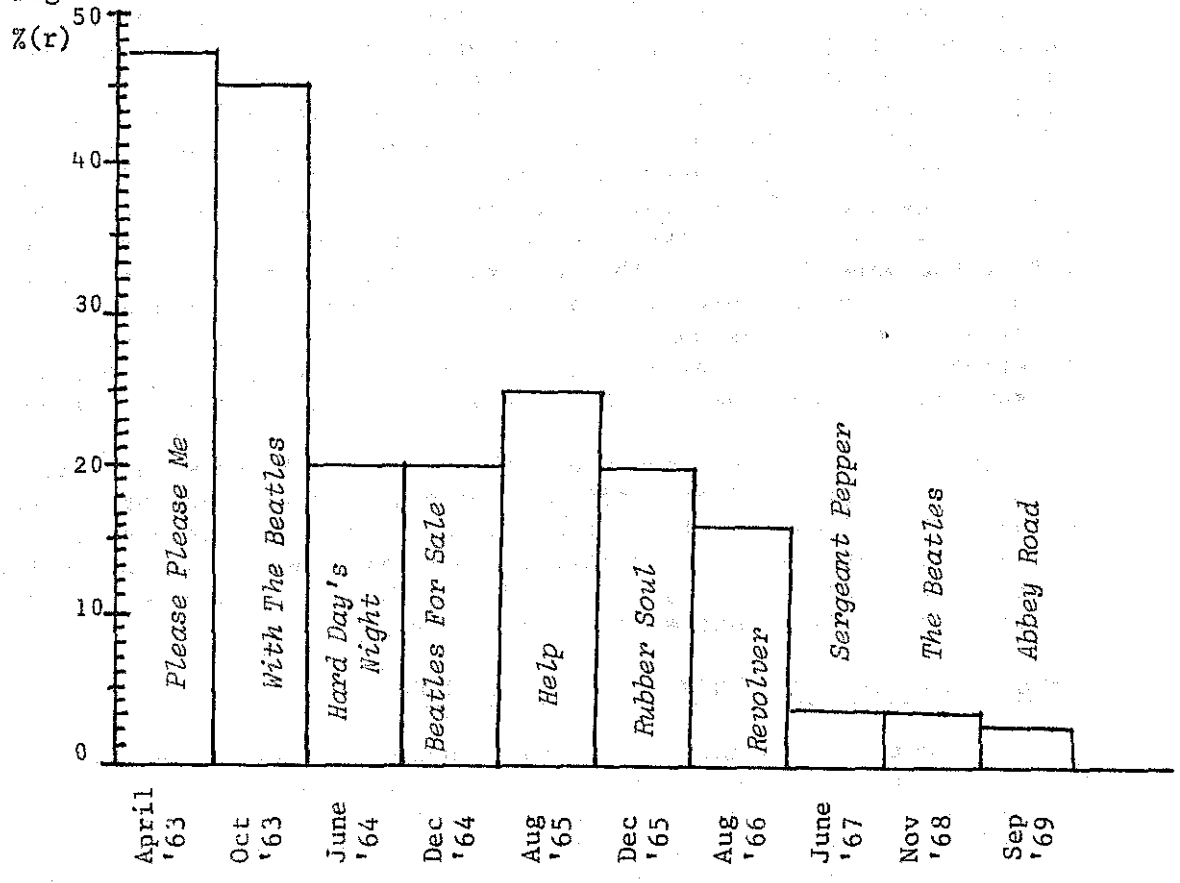
To count /r/s in this way is to acknowledge that, in British pop music, (r) is a linguistic variable in the Labovian sense. Employing the concept of the linguistic variable in this way, to examine this and the other features typical of British pop-song pronunciation, opens up the possibility of examining Le Page's theory in more detail,

especially insofar as it allows for the possibility of accounting for conflict and change.

It permits, for example, the quantitative comparison of the pronunciation of pop songs from the late 1950s and early 1960s with that of later periods. This turns out to be very instructive. For example, an analysis of four LPs from the period 1963-65 (the first two British albums produced by The Beatles and Rolling Stones respectively) gives an overall (r)-count, based on 372 tokens, of 36% (ie 36% of potential non-prevocalic /r/s were pronounced). On the other hand, analysis of four current (1978-79) British albums, selected at random (Dire Straits *Dire Straits*; Supertramp *Breakfast in America*; The Clash *The Clash*; Sham '69 *Hersham Boys*) gives an (r)-count, based on 546 tokens, of 4%. Obviously it would not be legitimate to draw any conclusions from such a haphazard and small-scale comparison. At the same time, it does suggest that something has happened to pop-song pronunciation, and it does tally well with casual observations to the effect that things have changed.

This is further reinforced by Figure 1. This figure portrays the (r) scores per album for ten of the eleven LPs released by The Beatles in Britain between 1963 and 1971, and paints rather a surprisingly dramatic picture. During The Beatles' recording life there was a very considerable falling-off in non-prevocalic /r/ usage. From a high of 47% in 1963 this falls to a low of 3% in 1970.

Figure 1



The same pattern is repeated in Figures 2 (i) and (ii). These graphs deal with the same ten Beatles records, together with scores for four records by The Rolling Stones.

Figure 2 (i)

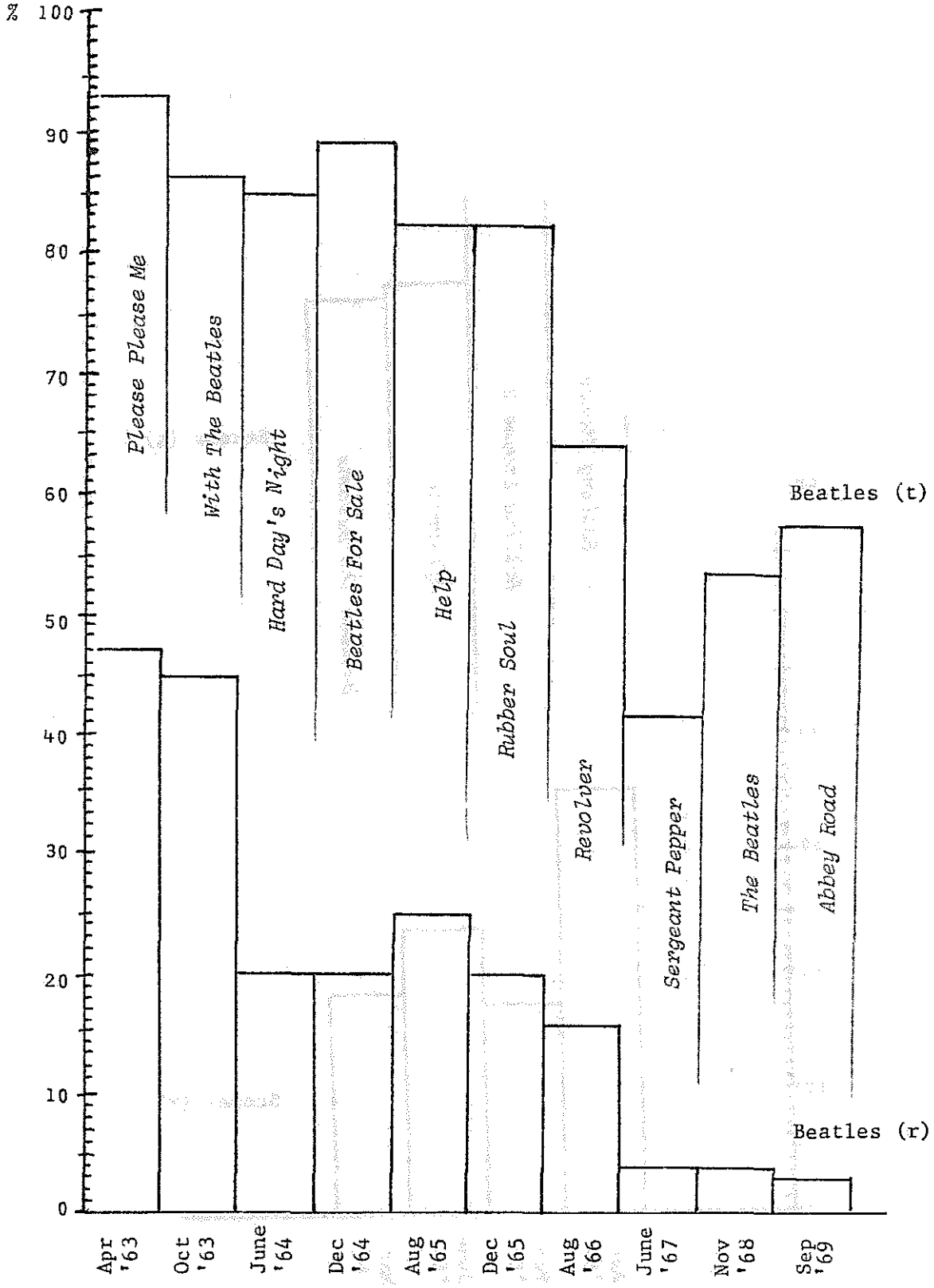
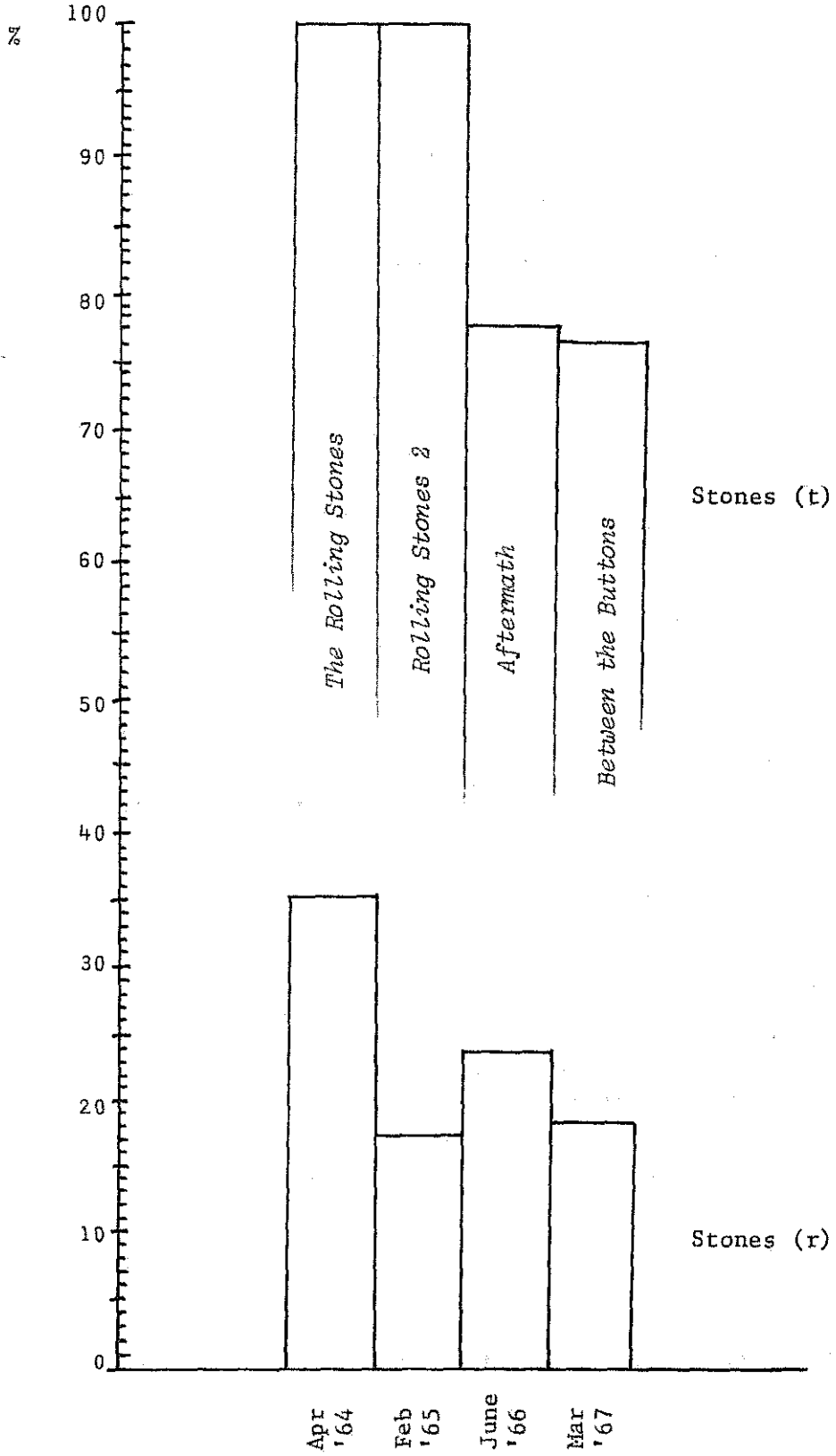


Figure 2 (ii)


%
100
90
80
70
60
50
40
30
20
10
0
The Rolling Stones
Rolling Stones 2
Aftermath
Between the Buttons
Stones (t)
Stones (r)
Apr '64
Feb '65
June '66
Mar '67

In addition to (r) scores, they also showthe percentage of intervocalic /t/s realized as [ɾ] = (t), and a less dramatic but equally clear picture emerges. (In calculating (t) scores, both environments such as *better* and *get a* have been included. The phrase *at all* has been omitted from calculations since British speakers, unlike Americans, have for the most part resyllabified this as *a#t all*. In the case of (t), the phonological environment which causes British singers most difficulty is as in *adversity, nobility*, where there is some variability even in American English.)

Other quasi-American features, too, can be shown to have declined in frequency. We can draw no conclusions from The Beatles' pronunciation of *dance, past* etc as their native northern (Liverpool) English accents have /æ/ in these words in any case. It is, however, possible to detect a change in their treatment of items such as *can't* and *half* which have /æ/ in the USA, but /a:/ in the north as well as the south of England. The progression is:

	Album	*can't, half*
1963	*Please Please Me*	/æ/
1963	*With The Beatles*	/æ/
1964	*Hard Day's Night*	/æ/
1964	*Beatles For Sale*	/æ/
1965	*Help*	/æ/
1967	*Sergeant Pepper*	/æ/~/a:/
1968	*The Beatles (White Album)*	/æ/~/a:/
1969	*Abbey Road*	/a:/

(The Rolling Stones, on the other hand, have always been /æ/-users, their lead singer Mick Jagger consistently scoring 100% on all albums.)

So what has happened? One factor that may be important in examining the performance of a group such as The Beatles is that it could be argued that their change in pronunciation reflects a change in genre. Their early songs are often clearly in the rock-and-roll mould, while their later songs tend to be more complex, contemplative, poetic, and so on. The subject matter of the songs changes, too, and the later songs, now written entirely by themselves, show an increase in more obviously British themes and locales. (It may also be of some relevance that songs written by British composers tend, naturally enough, to have *r*-less rhymes of the type *bought-short* (Beatles *Revolver*), *Rita-meter* (Beatles *Sergeant Pepper*) (cf Zwicky, 1976).) The particular 'image' that particular groups try to project will also be a factor.

But it is also clear that, from around 1964 on, British singers generally began trying less hard to sound like Americans. Why should this have been? Within the framework provided by Le Page, we can say that the strength of the motivation towards the American model has become weaker. If this is the case, then this change can in turn be ascribed to developments within the world of pop music itself. The enormous popularity of The Beatles, which extended to the USA by 1964, and, in their wake, of other Liverpool-based (and other British) groups, led to a change in the pattern of cultural domination. For a while, it was Britain that dominated America in this field, and,while this is no longer the

case, the *strength* of American domination was permanently weakened. British pop music acquired a validity of its own, and this has been reflected in linguistic behaviour.

CONFLICTING MOTIVATIONS

Le Page's third rider is:

> '(iii) the strength of various (possibly conflicting) motivations towards one or another model and towards retaining our own sense of our unique identity.'

As we have just seen, the strength of the motivation towards the American model diminished from 1964 on. Imitation of this model, however, was not in conflict with any other, and the American model remained the sole motivation there was for pop singers to modify their pronunciation. British singers were indeed trying *less hard* to sound like Americans; but it cannot be said that they were actually trying to sound *more British*. Nor does it actually appear to be true, although it was often claimed to be the case at the time, that from 1963 on singers from elsewhere tried to imitate a Liverpool accent. (The Beatles themselves sounded both more American, as we have seen, *and* more Liverpudlian on their early records than on their later records. Obviously Liverpool features included: the heavy aspiration/affrication of voiceless stops *(passim)*; the rhyming of *gone* and *one* (*With The Beatles*); the rhyming of *aware* and *her* (*Rubber Soul*); the pronunciation of *book* etc with long /u:/ (as late as *Sergeant Pepper*); the pronunciation of *hang a ...* and *long ago* with [ng] (*Revolver*).)

In more recent years, however, especially since 1976, this situation has changed. A new and conflicting motivation *has* arisen. This new motivation is most apparent in the performance of music by pop groups categorized under the heading of 'punk-rock' and/or 'new wave'. The music of 'punk' groups is typically loud, fast and aggressive, and the songs concerned, often, with themes such as violence, underprivilege, alienation, and rejection. The songs are also frequently - in non-punk mainstream societal terms - in bad taste. The intended primary audience is British urban working-class youth.

'Punk rock' singers, like their antecedents, modify their pronunciation when singing. Analysis of their pronunciation, however, shows that there has been a reduction in the use of the 'American' features discussed above, although they are still used, and an introduction of features associated with low-prestige south of England accents. These features, crucially, are employed in singing even by those who do not use them in speech. They include:

1) the use of wide diphthongs, as /ei/=[æɪ] *face*, and /ou/=[æʊ] *go*;
2) the pronunciation of /ai/ as [ɑɪ] *sky*, and of /au/ as [æu~ɛu] *out*;
3) the vocalization of /l/, as in *milk* [mɪʊk];
4) the (occasional) deletion of /h/;
5) the use of [ʔ] realizations of /t/ not only finally, as in *get*, but also intervocalically, where it is most socially stigmatized and conspicuous, as in *better*.

The use of these low-status pronunciations is coupled with a usage of non-standard grammatical forms, such as multiple negation and third-person singular *don't*, that is even higher than in other sub-genres of pop music, and the intended effect is assertive and aggressive. There is also clearly an intention to aid identification with and/or by British working-class youth, and to appeal to others who wish to identify with them, their situation and their values. The 'covert prestige' (see Trudgill, 1972) of non-standard, low-prestige linguistic forms is clearly in evidence, and the overall motivation, conflicting with that towards the American model, is clearly towards a working-class British model, and towards retaining, although at a group rather than individual level, a sense of a unique (and non-American) identity. (Note that accommodation theory - see above - might be applicable here.)

The continued use in punk rock of 'American' forms, however, shows that the assertion of a unique British working-class identity is not the whole story. The old motivation of sounding American has not been replaced by the new motivation, but remains in competition with it. Not only, for instance, are American pronunciations retained, but American locutions, such as *real good*, continue to be employed. Moreover, many obvious British features are not employed - intrusive /r/, for example, is extremely rare, even on recent recordings. And at points where the two pronunciation models are in direct conflict, such as the realization of /ai/, forms like [a·] and [ɑɪ] alternate, even in the same song.

We therefore have, in Le Page's terms, conflicting motivations towards different models - the American and the British working-class. This conflict, however, is not equally apparent in all types of British pop music. This is clearly revealed in Table 1. This table is based on data from seven recent albums only, and can again therefore be no more than suggestive. These albums are nevertheless possibly quite representative of those produced by many other singers and groups. The albums are: The Rolling Stones *Some Girls* (1978); Supertramp *Breakfast in America* (1979); Dire Straits *Dire Straits* (1978); Stranglers *Rattus Norvegicus* (1977); Clash *The Clash* (1978); Sham '69 *Hersham Boys* (1979); and Ian Dury *Do It Yourself* (1979). The Rolling Stones, Supertramp and Dire Straits represent the 'mainstream' pop music tradition, while the other performers represent the newer, punk rock oriented school. Information is given in the table on the percentage of non-prevocalic /r/s pronounced = (r); the percentage of intervocalic /t/s realized as [ɾ̪] = (t); the percentage of /æ/ rather than /a:/ in the lexical set of *path, dance* (except for Supertramp and Dire Straits, whose singers are from the north of England); and the percentage of intervocalic /t/s pronounced as a glottal stop. It can be seen from Table 1 that The Rolling Stones, so far as their pronunciation is concerned, are still at the no-conflict, semi-American stage typical of the mid-1960s (and indeed it has always been apparent that Mick Jagger has more self-consciously than most in many respects modelled his singing style on that of Black American rhythm-and-blues singers). The Stones' (r) and (t) scores are, however, still much lower than they would have been in the early 1960s.

Table 1: USAGE OF 'AMERICAN' AND 'BRITISH' FEATURES

	'American'			'British'
%	(r)	(t)	/æ/	[ʔ]
Rolling Stones	19	46	100	0
Supertramp	7	81	-	0
Dire Straits	1	92	-	0
Stranglers	0	88	80	0
Clash	6	71	24	10
Sham '69	1	57	50	9
Ian Dury	0	5	0	22

The two newer mainstream groups, Supertramp and Dire Straits, represent the phase after the weakening of American influence. This is apparent from a comparison of their (r) scores with that of The Rolling Stones: the very low (r)-count is typical of nearly all recent British singers. These two groups, however, still heavily favour the 'American' realization of (t) - unlike The Rolling Stones (their low score on *Some Girls* may be influenced by one particular song where the word *pretty* is repeated many times with [tʰ]). It seems, in fact, that British singers now have considerable freedom in the extent to which they feel obliged to conform to the earlier 'American' norm for (t). (It is perhaps relevant that the Supertramp record was recorded in the USA, is entitled *Breakfast in America*, and expresses sentiments such as 'Like to see America/ See the girls in California...'.) The supposedly 'punk' group The Stranglers also come out as having an orientation only towards the American model, in that they closely resemble the 'mainstream' groups in their linguistic behaviour. (This is probably of more interest to rock musicologists than to linguists, but it is interesting to note that The Stranglers have been one of the groups accused of having 'sold out' and of not being 'really punk'. Perhaps giving your records Latin titles does not help here either.)

The Clash and Sham '69, on the other hand, can clearly be placed, on phonological grounds alone, in the punk rock category. In their scores the conflict between the 'American' and 'British' motivations is clearly portrayed. Scores for (r) and /æ/ are low, and [ʔ] is quite heavily used (scores for word-final [ʔ] would be considerably higher than these intervocalic /t/ scores). And, interestingly enough, the orientation towards the 'British' model received overt recognition in one of the songs on the Clash album, which is entitled 'I'm so bored with the USA'. On the other hand, 'American' forms are still used extensively. Punk rock singing style is probably the only accent of English where the combination of *can't* /ka:nt/, *high* [ha•] and *face* [fæIs] is possible.

The scores given for Ian Dury are interesting in a different respect. These show no real signs of any conflict at all: his single model is clearly that of the speech of working-class London. Not too much, however, should be concluded from this as far as pop song pronunciation in general is concerned. Dury is in some respects

on the fringes of the pop-music tradition, and his bawdy and amusing lyrics owe perhaps as much to the comic tradition of the music-hall as to pop. His extensive usage of [ʔ], for instance, can certainly be attributed in part to the aggressive style of punk rockers, but it can also be attributed to the music-hall tradition which has often used Cockney pronunciation for comic effect. (A similar interpretation has to be placed on pronunciations used in The Kinks' 'Little bit of real emotion'(1979)where [lIʔɚ bIʔ əv] alternates, in the chorus, with [t^h] and [d_R] allophones of /t/. The effect of this is certainly not aggressive, and is perhaps more whimsical than anything else.)

CONCLUSION

British pop singers, then, have attempted to model their linguistic behaviour, when singing, on that of Americans. Their success in this has been constrained, first, by their ability to identify their model group: there has been some confusion as to which American accent it is that is being imitated. Secondly, their analytical ability has not always been sufficient to work out the rules of American linguistic behaviour, with the result that hyper-American /r/s occur. Thirdly, their ability to perform these modifications, even where analysis is correct, is far from perfect, so that American forms occur only inconsistently. Finally, as a result of the rise in the influence of British pop music, the strength of the motivation towards the American model was reduced, around 1964, and there was a drop in the frequency of occurrence of these American forms.

More recently, the past few years have seen the rise of a new sub-genre within pop music, often labelled 'punk rock'. Groups who perform this type of music typically cultivate a more aggressive, urban working-class image. This has led to the development of a motivation towards a new model: southern English urban working-class youth. This motivation has not replaced the motivation towards the American model, but coexists - and conflicts - with it. This conflict leads to a style of pronunciation where stereotypically American forms combine with stereotypically English working-class forms.

Le Page's work has already helped us to understand that the selection of linguistic forms from different codes may be due to split motivation, and that a combination of different linguistic features may be very functional in retaining a balanced public and self-image. In Belize, for example, the split may be between 'how to behave if you want eventually to get a job outside the country' and 'how to behave as a good Belizean' (see Le Page, 1978).

In British pop music, motivation towards different models have changed and shifted in the past two decades. The result is that the particular combination of 'American', neutral, and 'British' forms used may currently vary from singer to singer (and even from song to song). Singers in the mainstream pop tradition show for the most part motivation only towards the 'American' model, albeit much more weakly than was the case twenty years ago. A few singers,

like Ian Dury, may show an orientation almost exclusively towards a 'British' model.

Many punk rock singers, however, are like Belizeans. For them there is a genuine split in motivation. The conflict is between a motivation towards a supposedly American model, and a motivation towards a supposedly British working-class model - which can be glossed as a conflict between 'how to behave like a genuine pop singer' and 'how to behave like a British urban working-class youth'. The combination of linguistic forms that is typically found in punk rock singing is an attempt to find a balance between the two.

Le Page's theory is an attempt to find a general motive for speakers' linguistic behaviour. In developing this theory, Le Page finds it 'necessary to emphasise the phatic and self-expressive more than the communicative function of language'. Pop music is a field where language is especially socially symbolic, and typically low in communicative function, high on the phatic and self-expressive. It is not surprising, therefore, that the factors isolated in his work by Le Page are so clearly apparent in pop music singing. We must expect that in more typical and less restricted forms of language use, the location of these factors will be a much more complicated task. We must also expect, however, that it will be possible.

On the other hand, it is also certain that any inadequacies in the theory that are apparent in its application to pop-song pronunciation will also prove to be inadequacies elsewhere. One of these inadequacies is the following: even in the narrow field of pop-song pronunciation it is not possible, in terms of this theory (or any other), to explain why *particular* (in this case 'British' or 'American') consonantal, vocalic or other variants are retained, rejected or selected, and not others. We therefore await theoretical refinements, since it may be the case that until we are better able to account for why, say, The Clash (*The Clash* 'Remote Control') sing [kɑ:nt gɛɻ əˈhɛd] (rather than [kænt gɛɻ əˈhɛd] or [kɑ:nt gɛʔ əˈhɛd]), we as sociolinguists may perhaps not be able to make too much progress either.

FOOTNOTES

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2. There are other (eg choral) American singing styles which are deliberately *r*-less. These, however, have /aI/ as [aI], and are probably modelled on Eastern and/or English English accents.

REFERENCES

Giles, H and P Smith
1979 'Accommodation theory: optimal levels of convergence', in H Giles and R St Clair (ed.) Language and Social Psychology, Oxford: Blackwell.

Le Page, R B
1968 'Problems of description in multilingual communities', Transactions of the Philological Society: 189-212.

1975 'Polarizing factors: political, social, economic, operating on the individual's choice of identity through language use in British Honduras', in J G Savard and R Vigneault (ed.) Les Etats Multilingues, Quebec: P.U. Laval.

1978 'Projection, focussing, diffusion', Society for Caribbean Linguistics, Occasional Paper 9. Trinidad: University of the West Indies. Reprinted in this volume.

Le Page, R B, P Christie, B Jurdant, A Weekes and A Tabouret-Keller
1974 'Further report on the sociolinguistic survey of multilingual communities', Language in Society 3: 1-32.

Trudgill, P
1972 'Sex, covert prestige and linguistic change in the urban British English of Norwich', Language in Society 1: 179-96.

Zwicky, A
1976 'Well, this rock and roll has got to stop. Junior's head is hard as a rock', in S Mufwene *et al* (ed.) Proceedings of the 12th Annual Meeting of the Chicago Linguistics Society.

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